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VOL. XIX. No. 2 March, 1912

THE PSYCHOLOGICAL REVIEW

THE RELATION BETWEEN MODE OF PRESENTATION AND RETENTION

BY V. A. C. HENMON

University of Wisconsin

The relative value of the various methods of presenting material to be learned is a problem of considerable pedagogical and psychological interest. Presentation may be visual auditory, visual-auditory combined, visual-auditory-motor (articulatory or graphic), visual-motor (articulatory or graphic), or auditory-motor (articulatory or graphic). In view of the possible practical application of the results in teaching, the problem has been frequently taken up, particularly the comparison of the values of visual and auditory presentations for immediate memory and the influence of motor factors. Experiments on visual and auditory memory have been reported by Münsterberg and Bigham (18) (method of reconstruction); by Kirkpatrick (12), Hawkins (9), Quantz (21), Smedley (25), Calkins (3), Wissler (29), Netschajeff (19), Kemsies (11), Lobsien (15), W. A. Lay (14), Pohlmann (20), Aliotta (2), Schuyten (23) (method of amount retained or the method of memory span); and by Whitehead (28), Fränkl (7), Segal (24), and von Sybel (29) (committing method). The results of the experiments are not in accord. It is generally claimed that with younger children auditory presentation gives the better results, except for meaningless material, while in older children and adults visual presentation is better than the auditory. This result is attributed to the fact that young children are less familiar with written than with spoken language, that oral methods of teaching

are gradually superseded by methods that make appeal to vision and visual imagery, and that auditory presentation induces greater concentration of attention. The change in relative value of auditory and visual presentation may also be due to an actual change in image type with age. Meumann (16), however, states that visual presentation is far easier for learning both for children and adults and that this rule holds for all material, though more especially for nonsense-syllables.[1] The rule is limited only by type of imagery. Elsewhere, Meumann (16) states that a single method of presentation for all cases can not be declared to be most advantageous.[2] The value of a method of presentation varies with the nature of the material, the type of imagery of the learner and the procedure in presentation.

Less attention has been given to the effect of simultaneous presentation to several senses and to the relation of methods of presentation to secondary memory. On general psychological grounds it might be expected that combining simultaneously various methods of presentation would be an advantage, particularly with stimuli, such as words, which could, if necessary, be learned naturally through visual or auditory presentation. There would supposedly result an increase in the number of associations, which would facilitate retention and recall. If the visual cue is lacking, the auditory or the motor cue might be effective. On the other hand, too great an increase in the number of possible cues may be distracting and reduce the force of associations. This might be expected where the natural appeal is to one sense, as in colors or tones.

The results of experimental investigation are inconclusive. Münsterberg and Bigham (18), found that "a series of presentations offered to two senses at the same time is much more easily reproduced than if given only to sight or only to hearing." The percentages of error in reconstruction of series of numbers and colors were as follows:

[1] P. 25.
[2] P. 121.

	Per Cent.
10 numbers heard	14.1
10 numbers seen	10.5
10 numbers seen and heard at the same time	3.9
10 colors named	29.3
10 colors seen	17.9
10 colors seen and named at the same time	4.9

There is thus a significant superiority in the combined method. When taken alone "visual memory excels strongly the aural."

Quantz (**21**), however, determined the visual and auditory memory-spans for words and notes that "the use of eye and ear together, the words being read aloud by the subject, is little advantage over either separately, when the words are read to him or silently by him." In fact, the combined presentation may be a hindrance.

W. A. Lay (**14**), in several researches on methods in teaching spelling, has studied the influence of kinesthetic factors (articulatory and graphic) combined with visual and auditory presentation in apprehension and retention of nonsense syllables and numbers. In all cases the suppression of articulatory movements increased the errors, while the use of graphic or articulatory movements reduced the errors, an effect which might be due to the indirect influence on attention rather than the direct influence of the kinesthetic factors themselves.

Fuchs and Haggenmüller (**8**) and Itschner (**10**) repeated Lay's experiments under more constant conditions and did not find that articulatory or graphic movements played the important part in learning which Lay attributed to them. Abbott (**1**) in a study of memory consciousness in orthography finds that "irrespective of the method of presentation and the manner of learning, the typical mode of recall for all observers was through the visual imagery of the letters." Vocalization is always used and aids in learning the spelling of a word by determining the correct pronunciation of it and thus arousing auditory imagery and by facilitating accurate visual perception.

Cohn (**4**) tested the coöperation of visual, auditory and

motor factors, with especial reference to motor factors, in the memorizing of lists of twelve consonants. In one set of experiments subjects' read the consonants aloud, in the second speech movements were suppressed, and in the third numbers or vowels were pronounced during the reading of the consonants. All the subjects remembered most by the first method and least by the last. Cohn's results are thus similar to those of Münsterberg and Bigham.

Smedley (**25**) in tests of immediate memory for digits on Chicago school children found auditory-visual memory better than either alone, that the visual-auditory-motor (articulatory) was better than the auditory-visual, and that the visual-auditory-motor (graphic) was inferior to both the auditory-visual and the visual-auditory-motor (articulatory).

Kensies (**11**) tested auditory, visual and visual-auditory presentations of Latin words and nonsense words with school children and found the auditory presentation to be best in all cases. The combined method usually gave poorer results than with visual or auditory presentation alone.

Finzi (**6**) noted that in visual presentation of letters, numbers and nonsense-syllables the subjects might employ for retention visual images alone, or the auditory and articulatory images combined, or the articulatory images alone. He found that retention by means of visual images gave the most reliable results.

Pohlmann (**20**), in the most complete investigation of sensory modes of presentation thus far by the method of amount retained, studied the effect of visual, auditory, visual-auditory and visual-auditory-motor presentations of objects, words, nonsense-syllables and numbers on school children from 9 to 14 years of age. He found that auditory presentation is better than visual with significant material (words) but that the visual presentation is better with nonsense material (numbers and syllables). The value of visual presentation for words increases with age and finally surpasses the auditory. The combined visual-auditory presentation shows on the average in all cases a slightly better result than with the auditory or the visual alone. The visual-auditory-

motor presentation gives poorer results with three repetitions and with one presentation very much poorer results. His percentages of amount retained, omitting the results on objects, are as follows:

	Auditory	Visual	Visual-Auditory	Visual-Auditory-Motor
Words	55 1/6	50 4/6	56 1/2	49 1/2
Nonsense syllables	42	53 32/60	53 51/60	52 5/16
Numbers	47 1/4	54	54	51 5/12

Fränkl (**7**) using the committing method with eight syllable series tested the values of auditory, auditory-motor, visual-auditory-motor and visual presentations. He found that the visual presentation was better with visual types, auditory with auditory types.

Schuyten (**23**) in class experiments with eight two-place numbers found auditory presentation better than visual-auditory.

Segal (**24**) found that visual presentation was best for those of visual type, auditory presentation for the auditory types. Meumann (**17**) concludes from his own observations and those of Segal that, in learning, reliance upon the natural type of imagery is better than the use of a combined method. He points out that it is not the number of associations but the strength of associations that determines retention and recall.[1]

Von Sybel (**27**) in a recent study compared visual, visual-motor, auditory, auditory-motor, visual-auditory and visual-auditory-motor presentations using a combination of the committing method and the method of right associates. The experiments were made on seventeen subjects with nonsense-syllables. He finds that reading aloud (visual-auditory-motor) is better for learning in almost all cases than silent reading (visual). This result holds even with those of visual image type except with slow rates of presentation where silent reading gives best results. The number of right associates, however, is practically always greater with silent reading, regardless of image type.

[1] P. 88.

Visual-auditory presentation is almost without exception better for learning than the visual but retention is better with visual presentation. All subjects considered visual-auditory presentation to be distracting and expected poorer results, an expectation which was not borne out. Visual presentation with articulation gave slightly better results for learning than without articulation but right associates were more numerous when articulatory movements were suppressed. Auditory presentation with articulation gave practically the same result as without articulation. Auditory presentation gave without exception better results than the visual. This was true both for auditory types and also for the dominantly visual.

This summary of the available evidence bears out the statement that the results on the effect of methods of presentation on learning and retention are not in accord.

The early experimenters apparently assumed that tests with various methods of presentation measured the efficiency of the visual, auditory and motor memories. However, it is clear that the method of presentation is not necessarily an index of the imagery employed. The method of learning depends in part on the method of presentation, in part on the sort of material and in part on the imagery of the learner. A list of words may be read to three subjects and if they represent three distinct types of imagery the audile will recall the list by auditory images, the visual will translate the words into visual images and the motor will speak the words internally. Since the great majority of individuals are of the mixed type of imagery, using now one form of imagery and now another, the memory-images employed will vary with the sort of stimuli used, the mode of presentation and the degree of dominance of one form of imagery in this mixed type. Visual stimuli will, other things being equal, be better remembered visually, auditory stimuli by auditory images. The mode of presentation may thus determine the method of learning. Similarly the nature of the material may determine the method of learning.

In view of these considerations it is necessary to distinguish

between the psychological and the pedagogical problems involved. Pedagogically the problem is largely quantitative. How much can be retained by each method of presentation and how accurate is the retention? The interest is in the results of the presentation rather than in the processes by which the presented material is retained. Psychologically the problem is largely qualitative. How is the material apprehended, retained and reproduced and how does the nature of apprehension, retention and reproduction vary with the type of imagery, the mode and rate of presentation and the sort of material used. In experiments, as Meumann (17) points out, we may approximate as closely as possible actual school conditions but in such cases we are not in a satisfactory position to study the processes employed in learning. Or we may instruct subjects to use auditory imagery with auditory presentation, visual imagery with visual presentation, etc., but then we may be interfering with the natural method of learning. In the first case we discover how the various modes of presentation actually affect learning but the analysis and explanation of the results is difficult. In the second case we get definite results of interest to psychology but the practical application to school conditions is doubtful.

Experiments and Results

The following experiments were designed to test the influence of visual, auditory, visual-auditory and visual-auditory-motor (articulatory) presentations on retention. Since the value of a method of presentation may vary with the kind of material used, with the number of repetitions, and with different individuals, the experiments were made with three sorts of material, concrete nouns, two-place numbers and nonsense-syllables, with one, two and three repetitions and on six subjects. Precautions were taken to secure, as far as possible, uniformity in the material. The nouns were all of four letters each, arranged so as to avoid placing words in succession that were similar in sound or appearance. No word began with the same letter which was found either at the beginning or end of the word preceding it.

In the lists of numbers no consecutive numbers began or ended with the same figure. The same figure was not used twice in any one number; the zero was not used at all; and all multiples and divisors of a number in any list were avoided. The usual precautions were taken with the lists of nonsense-syllables. To avoid difficulties in the case of auditory presentation syllables beginning or ending with c, q, and h, and syllables beginning with x, were rejected. The syllables were all of three letters.

The method employed was that of amount retained (Methode der behaltenen Glieder). Each series consisted of ten members, typewritten on strips of paper which could be fastened around the drum of a kymograph. The rotation of the drum behind the screen before which the subjects were seated was kept at a uniform rate and permitted the exposure of each member of a series for three quarters of a second with an interval of one and one half seconds between successive members. In the visual presentation the subjects read the stimuli directly from the rotating drum and immediately wrote down as many members as could be recalled and in the order presented. The subjects were asked to repress movements of articulation. In the auditory presentation the experimenter read the stimuli from the drum, the subject keeping his eyes closed and repressing movements of articulation. In the visual-auditory presentation the subject both saw the stimuli and heard them read by the experimenter. In the visual-auditory-motor presentation the subject himself read the lists aloud. A double fatigue order was observed in number of presentations and in order of materials and in modes of presentation.

Six subjects took part in the experiment. All had had a year or more of laboratory training in psychology. I am particularly indebted to Mr. Carl L. Rahn, sometime instructor in psychology at the University of Colorado, for his assistance both as subject and as experimenter, and to Miss Mary E. Lakenan, assistant in psychology, at the same institution, for aid both as subject and in the calculation of the results. The experiments were made during the year

Table I

One Presentation

Subjects	Nouns								Syllables								Numbers							
	V.	m.v.	A.	m v.	V.A.	m.v.	V.A.M.	m.v.	V.	m.v.	A.	m.v.	V.A.	m.v.	V.A.M.	m.v.	V.	m.v.	A.	m.v.	V.A.	m.v.	V.A.M.	m.v.
A	42.7	9	59.5	10	53.3	8	56.4	6	17.4	9	36.0	8	32.1	11	30.5	9	19.9	7	27.4	3	31.3	5	33.2	8
B	48.0	8	60.0	11	59.7	14	57.3	6	38.2	6	37.4	9	34.6	7	34.1	4	56.0	17	54.2	11	53.8	12	58.0	15
C	45.3	8	57.2	5	50.5	10	61.6	7	25.5	5	35.1	9	41.3	9	38.9	6	40.7	7	45.7	11	44.3	10	44.0	9
D	50.7	9	60.9	9	68.1	13	68.7	12	28.8	6	37.1	6	35.9	9	40.1	5	25.6	7	33.6	7	36.6	5	33.3	9
E	23.8	5	35.1	4	36.7	5	30.4	5	13.6	6	18.1	5	19.4	5	15.8	4	28.2	7	38.9	10	32.6	5	32.8	5
F	41.7	10	47.8	4	56.5	10	50.1	9	26.9	8	35.5	4	35.7	8	36.2	8	47.8	11	56.1	14	49.5	8	44.9	9
Av.	42.0	8.1	53.4	7.2	54.1	10.0	54.0	7.5	23.4	6.7	33.2	6.8	33.1	8.2	32.6	6.0	36.3	9.3	42.6	9.3	41.3	7.5	41.0	9.1

Two Presentations

A	56.8	17	72.3	13	70.0	8	67.4	14	31.3	7	50.1	12	43.6	10	34.7	8	22.6	9	35.7	7	28.4	6	33.7	7
B	62.8	13	71.1	14	70.0	11	74.8	14	39.7	11	40.7	9	45.0	15	41.1	14	57.5	15	66.0	11	69.4	15	60.1	12
C	45.6	11	64.7	3	62.0	9	60.6	8	32.0	8	38.6	9	35.3	10	39.3	7	45.7	12	54.2	12	47.4	12	52.1	11
D	77.9	17	73.9	15	90.9	9	74.1	13	30.9	11	31.6	6	35.6	8	45.6	10	32.4	6	41.3	11	44.9	8	38.7	7
E	38.4	8	43.7	6	48.9	7	39.3	9	19.0	7	22.5	10	17.9	4	17.1	5	34.1	7	35.0	7	39.8	8	36.8	7
F	61.1	12	59.5	17	75.5	12	68.9	10	37.2	4	36.0	6	41.5	5	40.6	7	48.9	7	55.8	14	55.5	11	54.6	20
Av.	57.1	13.0	64.2	11.3	71.2	9.3	64.1	11.3	31.7	8.0	36.5	8.7	36.4	8.7	36.4	8.5	40.2	9.3	48.0	10.3	47.5	10.0	46.0	10.6

Three Presentations

A	64.1	12	70.0	10	84.4	8	79.8	11	32.2	13	44.8	11	41.2	14	45.4	10	26.2	10	36.1	9	33.5	9	34.9	9
B	81.5	11	78.2	11	80.8	13	80.9	13	43.8	11	44.8	8	45.6	12	54.3	18	67.3	15	78.2	12	75.1	16	76.0	14
C	50.3	10	68.7	10	63.4	7	64.0	11	28.9	7	45.7	10	41.2	6	44.7	5	50.6	11	60.9	8	54.6	9	52.9	9
D	84.3	10	71.9	10	85.9	14	80.2	17	43.3	18	39.5	14	42.4	19	59.5	13	49.8	11	46.5	13	46.7	13	44.9	7
E	40.6	7	51.1	12	51.5	5	41.9	7	16.0	4	29.0	6	25.0	7	23.2	6	38.4	11	58.8	14	37.8	6	43.7	8
F	63.9	15	71.4	12	66.6	10	71.4	14	31.5	12	41.5	9	45.3	8	42.7	12	55.8	8	68.0	12	58.4	10	55.4	9
Av.	64.1	10.8	68.6	10.8	72.1	9.5	69.7	12.2	32.4	10.7	40.8	9.7	40.1	11.0	43.4	10.7	48.0	11.0	58.0	11.3	50.5	8.8	51.3	9.3

1908–1909. All of the subjects were of the mixed type of imagery, *A*, being markedly auditory-motor, and the remainder very markedly visual.

The gross results of the experiments appear in Table I. It gives in summary the average percentages of the series retained for one, two and three presentations, for the three sorts of material used, and the six subjects, *A*, *B*, *C*, *D*, *E*, and *F*, the mean variations and the general averages for the six subjects. Each figure in the table, except the general averages, represents an average from 10 experiments. The figures, therefore, possess a high degree of reliability, the maximum probable error being .056 and the minimum .008, with an average probable error of .027.

1. Comparison of Visual and Auditory Presentations

The most striking result of the experiments is the marked superiority of the auditory over the visual presentation. This result holds in all but six of the fifty-four cases shown in the table, *B* giving a slightly better average for visual presentation of syllables and numbers with one presentation, and *D* giving a better average for visual presentation of all materials with three presentations. *B* and *D* are markedly visual in image type. All of the remaining subjects, except *A*, are also visual. The result is, therefore, surprising and not easily accounted for.

The fact that subjects of the visual type retain more with auditory presentation indicates either that image type is not a significant factor in determining the value of the mode of presentation, or that visualization and retention are more accurate when stimuli are heard than when read. The latter alternative seems the more probable. The introspections show that many subjects of the visual type tend always to visualize the stimuli and that greater freedom in visualization is possible with auditory presentation. Visual presentation is distracting and puts a constraint on visual imagery. This is especially true when stimuli are presented on a rotating drum, which increases the difficulty for the

visualizer of arranging a series quickly and without confusion. If the stimuli were exposed successively in different locations in the field it seems likely that the visual presentation would show to better advantage. This appears to me to account in part for the superiority of auditory presentation in my results and in those of von Sybel. The method of presentation ordinarily employed in memory experiments, for visual presentation is more artificial than for the auditory, and hence tends to favor auditory presentation. Moreover, the influence of the constant practice acquired by adult students in retention from auditory presentation is an important factor.

2. Comparison of Visual and Auditory Presentation with Different Materials

The superiority of the auditory presentation over the visual holds for all the materials used, being quite as great for nonsense-syllables and numbers as for nouns. This result does not accord with that of Pohlmann, who found, as indicated above, that the visual is much superior to the auditory presentation with nonsense-syllables and numbers but inferior with nouns. He found, moreover, that even with nouns the relative value of auditory presentation decreases with age and that ultimately visual presentation surpasses the auditory with nouns also.

Pohlmann used three presentations only with nonsense-syllables. The stimuli were presented at intervals of two seconds. His experiments were class tests on school children who had no familiarity with nonsense-syllables. Moreover, the syllables included diphthongs and digraphs and hence were more difficult of utterance and more difficult to apprehend from oral presentation. This difference in conditions no doubt accounts for the difference in results.

Class tests, such as those of Pohlmann, seem to me to have little value. Pohlmann points out the obvious reasons why the experiments throw little light on the psychological processes involved, but suggests that they may have a pedagogical value. But the pedagogical value of methods of presentation must be determined not by a single test on groups

of individuals but by repeated tests on the same individual. From a pedagogical point of view we are interested in the best and most economical mode of presentation shown after practice and in the long run. A single test with nonsense-syllables on school children, to whom such stimuli are unfamiliar, may show visual presentation to be superior, with ten tests or more the auditory may be superior and pedagogically this would be the fact worth knowing. It seems likely from some brief experiments which I have made with children of the same age as in Pohlmann's experiments that the superiority of auditory presentation would be shown with them also.

Kuhlmann's (13) review of the evidence leads him to the conclusion that "visual presentation of meaningless material is always better than auditory presentation." However, the careful study of Kemsies (11) showed the superiority of auditory presentation quantitatively and qualitatively both for Latin words and nonsense-words. Hawkins (9) found auditory presentation of words superior with children. Von Sybel (27), likewise, found that the number of repetitions required for learning was less for auditory presentation. My own results point clearly to the superiority of auditory presentation.

3. Visual-Auditory Presentation

The visual-auditory mode of presentation, where the stimuli are presented simultaneously to the eye and the ear, is superior to the visual presentation alone in all but seven out of the fifty-four cases, or 87 per cent. In the general averages the superiority holds in all cases. The combined method is superior to the auditory alone in but twenty-five of the fifty-four cases, or 46 per cent. In the general averages the combined method gives better results with nouns but with syllables and numbers there is practically no difference, the combined method being slightly inferior. This result is in substantial agreement with that of Pohlmann and other recent investigators. Von Sybel found that the number of repetitions required for learning was less with visual-auditory

presentation than with visual presentation but that the number of right associates (Treffer) was greater with visual presentation. His data do not enable him to compare visual-auditory with auditory presentation. His subjects noted the distraction arising from the twofold division of attention, from the fact that the visual and the auditory presentations were not simultaneous, and from the differences in pronunciation of the syllables. In spite of these distractions, which led most of the subjects to feel that visual-auditory presentation was less effective than the visual alone, the results showed an advantage in favor of the visual-auditory method.

4. Visual-Auditory-Motor Presentation

Visual-auditory-motor presentation, in which the subject himself reads the stimuli aloud, is inferior to the visual-auditory in thirty-two out of fifty-four cases, or 59 per cent.; to the auditory alone in thirty cases, or 55 per cent.; and to the visual alone in eight cases, or 15 per cent. In the general averages the differences between the visual-auditory-motor, the visual-auditory, and the auditory presentations are very slight, while all are superior to the visual alone. Simultaneous appeal therefore to the several senses is no advantage for retention. This result is in agreement with that of Pohlmann. Pohlmann, however, apparently attributes the inferior results to the distraction caused by the simultaneous speaking of the pupils in the class and not to the lack of reinforcement by motor factors. That this is not the true reason is shown by the fact that in my experiments the subject alone read the stimuli aloud, and this possible factor is eliminated.

The studies made by Cohn, Smedley, Lay, Smith, Aliotta and von Sybel have all shown the superiority of the visual-auditory-motor (articulation) presentation over other methods, and it is generally claimed that vocalization of what is to be learned is an aid to memory. Pohlmann, Colvin, Fuchs and Haggenmüller find the value of articulation to be slight. Colvin (**5**) concludes that "the importance of motor

imagery, both for the hand and for the vocal organs, appears to be much less than has generally been supposed. . . . Except in pronounced cases where the child is extremely motor in his way of thinking, children seem to depend but little on their motor imagery; indeed, the kinesthetic sensations from the throat and hand may be a hindrance rather than an aid in learning." This conclusion the writer would extend to adults so far as immediate memory in relation to articulatory sensations is concerned. Vocalization may be an aid in inducing attention when there is a tendency for it to wander and may bring about clearness of perception of details with unfamiliar material. Where this is unnecessary vocalization is distracting and of no assistance in memorizing.

5. Influence of Repetitions

The relative value of the different modes of presentation remains unchanged for one, two and three presentations.

The auditory presentation is superior to the visual in all cases. The difference in amount retained for nouns are 11.4 per cent., 7.1 per cent., and 4.4 per cent., respectively; for syllables, 9.8 per cent., 4.9 per cent., and 8.4 per cent.; and for numbers, 6.3 per cent., 7.8 per cent., and 10 per cent. It seems, therefore, that the value of the auditory presentation is greater for one presentation with nouns and decreases with an increase in repetitions while with numbers the reverse is the case.

With all of the other forms of presentation there is practically a uniform increase in amount retained with an increase in the number of presentations, and the relative values are constant for all materials. There is no evidence in support of Hawkins' conclusion that the second presentation gives a poorer result than one. In seven cases out of seventy-two a slightly better result was obtained with one presentation than with two.

6. Individual Differences

The individual differences in amount retained are considerable even in such a highly selected group, the range

being approximately as 2 : 1. The differences are practically constant for the various modes of presentation contrary to expectations. If one averages the percentages retained for visual, auditory, visual-auditory and visual-auditory-motor presentations separately, combining the results with the different materials and with the three presentations, which would serve as a rough measure of the individual's performance, or better still, if one averages the ranks attained under these conditions, the stations of the six individuals are practically identical for the four methods of presentation. In other words, the correlations in abilities with the different forms of presentation are practically perfect. Superiority with one form of presentation means practically the same degree of superiority with others. This result is contrary to a common belief that superiority with one form of presentation is correlated with inferiority or a much lower degree of superiority in others. The closeness of the correlation is no doubt due largely to the fact that with practiced adults the natural method of learning is the same no matter what the form of presentation may be. Image type is a factor of some influence as is shown by the record of subject *A*, who is auditory-motor in type and whose station is relatively better with the auditory than with the other presentations.

The correlation between abilities with different materials are not as close as might be expected, for, after all, the differences for learning between nouns, syllables and numbers are not great. The main facts can easily be seen from a table of the ranks, for the different individuals, based on the average of all percentages for nouns, syllables and numbers separately, or on an average of the ranks attained in the different experiments.

	Nouns	Syllables	Numbers
A	3	5	6
B	2	1	1
C	5	4	3
D	1	2	4
E	6	6	5
F	4	3	2

The correlations between syllables and numbers and between nouns and syllables is high, the coefficient of correlation by the method of rank-differences being in each case +.77. The coefficient for nouns and numbers is +.20. The number of cases is too small to attach much significance to the figures but they represent roughly the amount of correlation. Subject *D* is clearly of the so-called ingenious type and her record with nouns is much superior to that with nonsense-syllables and numbers, the absence of associations with numbers causing especial difficulty.

7. Conclusions

The following summary sets forth the main conclusions of this study:

1. Auditory presentation is clearly superior to visual presentation in immediate memory of adults, a result attributable to the greater ease and freedom of visualization with auditory presentation and the greater effort of attention required.

2. This superiority of auditory over visual presentation holds for all materials (nouns, nonsense-syllables, numbers), for all subjects irrespective of image type, and for one, two and three presentations. This result is not in accord with the opinion commonly held that visual presentation is superior, especially with meaningless material.

3. Combined visual-auditory presentation is slightly inferior to the auditory alone and decidedly superior to the visual alone. The advantage of a combined method is very much less than that shown in earlier investigations.

4. Visual-auditory-motor presentation is slightly inferior to the auditory and the visual-auditory presentations and superior to the visual alone. Articulation or vocalization is of little value for immediate memory.

5. The correlations of abilities with different forms of presentation are positive and very high, superiority with one indicating practically the same degree of superiority with another.

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COMBINING THE RESULTS OF SEVERAL TESTS: A STUDY IN STATISTICAL METHOD

BY R. S. WOODWORTH

Columbia University

When several tests have been made of the same individuals, it may be desirable to combine the results so as not only to get group averages and coefficients of correlation, but also to show the success of *each individual* in the series of tests taken as a whole. A prevalent custom, in such cases, is to drop from quantitative to qualitative statements, and to say, for example, that an individual who has done very well in the first test, well in the second, but rather poorly in the third, has on the whole, therefore, done rather well. A somewhat more quantitative statement can be got by ascertaining in what proportion of the tests an individual stands above the average, and a still better plan is to transmute the original measures into an 'order of merit' for each test, so as to be able to state, for example, that an individual stood first in one test, fifth in another and fifteenth in a third, and had accordingly an average rank of seventh. This method (in the hands of Cattell and others) has proved of much value where absolute measures are impracticable; but to transmute absolute measures, already obtained, into an order of merit is to throw away part of the information contained in the measures. What is needed is a method of combining results which shall preserve all the refinement of the original measurements. Such a method exists, and is certainly familiar to statisticians; but it seems to be overlooked in many cases where it would prove of value.

What is here attempted is (1) to win favor for the method (credit for its invention being expressly disclaimed); and (2) to work out simplified formulæ which can be used for computing correlations, when the method has been employed. The method itself gives each individual's average standing in any

number of tests; once this has been found, the additional labor of computing correlations is slight.

I

The problem, once more, is as follows: results being at hand from several tests of the same individuals, it is desired to combine them so as to measure the success of each individual in the tests as a whole.[1] This may be difficult, either because the measurements afforded by the different tests are imcommensurable (one being, perhaps, in terms of time and another in terms of accuracy), or because the average for one test is very different from that for another test.

Suppose, for example, that the group average for one test is 10 seconds, and for another test 100 seconds; and suppose that an individual's records in the two are respectively 8 and 70 seconds. How shall we combine his records? If we simply take the average of 8 and 70 seconds, we obtain a hybrid value with so great a probable error as to make it useless for further deductions. If we express each of his records in

[1] Essentially the same problem may arise in other forms. The general problem may be stated as follows: We have several series of measurements, and some principle of correspondence which enables us to connect each measurement in one series with one and only one measurement in each of the other series; and it is desired to combine the corresponding measurements. The writer has met this problem in several different forms. On one occasion I desired to find the relative difficulty of a number of words as stimuli for the 'opposites test.' I tried the words on several individuals, obtaining the association time for each word. Now as some of my subjects were much slower than others, and varied much more from word to word, simply averaging the association times for each word would not do equal justice to all the individuals, but the resulting differences between the words would be determined mostly by the slow, variable individuals. I desired some method of giving each individual, whatever his speed and variability, the same share in determining the average result; and I was finally led to the method described in the text. Again, in studying the work curve for very short periods of activity, I made use of lists of twenty words, the subject being required to respond to each word by a word standing in some assigned logical relation to it, and the time being taken for each single reaction. The subject went through twenty such lists, all different, and I desired to find whether the position of a reaction in the series of twenty affected its speed. But the lists of stimuli were, unavoidably, of unequal difficulty, and the variation within one list was greater than in another. If, then, I simply found the average reaction time for the first words, etc., and compared these averages, my comparison would not be based equally on all the experiments, but mostly on those that showed the greatest internal variation. What I desired was a means of giving each experiment an equal influence on the total result; and this was accomplished by the method given in the text.

terms of the corresponding group averages, as 80 and 70 per cent. respectively, we seem, indeed, to avoid most of the spurious unreliability, and we seem also to find that the individual did better in the first test than in the second. But this procedure is not justified, except on the assumption that the variability of a group in different tests is proportional to the group average; for, otherwise, a good share of the group may, in one test, take less than 70 per cent. of the average time, whereas in another test only an exceptionally good record may be as as low as this, so that such a mark as 70 per cent. of the average may mean very different degrees of proficiency in different tests.

Now, as a matter of fact, there is no general law that the group variability is proportional to the group average. Thorndike[1] has shown that no uniform relation holds be ween the verage and the variability, and has brought forward, in particular, cases in which the variability increases more slow'y than the average. When the length of a test, consisting of a series of similar reactions, is increased, the variability does not increase as fast as the average, because chance variations in the elementary reactions tend more and more to compensate for one another as their number is increased. But suppose—and this is a point not hitherto made—that not the length of the series of reactions, but the difficulty of the single reactions is increased—then one would expect the variability to increase faster than the average.

One would expect adults to differ proportionately more in long division than in easy addition, for all would have kept up some practice in the latter, while in the former some would still be in practice and some entirely out of practice. If, indeed, the difficulty of the test were still further increased —imagine, for example, a test in extracting the cube root— both the variability of the group and the average (as measured by time of performance) would be indefinitely increased because of the presence of some individuals who would fail utterly; and in this case the proportionate variability could not be calculated. But within a moderate range of difficulty

[1] 'Empirical Studies in the Theory of Measurements,' 1907, p. 9.

of the test, the variability should increase more rapidly than the average time of performance. The following table of results from a series of tests[1] tends to favor this conclusion, since, within each class of tests, the easiest have the lowest proportionate variability. Certainly the proportionate variability differs greatly from one test to another, and accordingly any statistical treatment which assumes equal relative variability is inaccurate.

	Group Average Time per Single Reaction	σ	$\frac{\sigma}{\text{Av.}}$
1. Logical relations:			
Opposites	1.23	.17	.14
Agent-action	1.30	.23	.18
Verb-object	1.39	.21	.15
Part-whole	1.53	.37	.24
Attribute-substance	1.53	.47	.31
Supraordinate concept	1.54	.37	.24
Action-agent	1.55	.41	.27
Whole-part	1.57	.36	.23
Subordinate concept	1.84	.42	.23
2. Mixed relations	3.14	.53	.17
3. Addition, Kraepelin test	1.14	.26	.23
4. Constant increment: Add 4	1.36	.30	.22
Subtr. 4	1.64	.54	.33
Add 17	3.90	1.18	.30
5. Color naming	.59	.10	.17
Form naming	.87	.18	.21
6. Substitution	1.60	.21	.13
7. Number-group, cancel 3	.79	.06	.08
Number-group, cancel 28	1.14	.10	.09

To return now to the problem of discovering a method of combining the individual's records in several tests: There is a way of eliminating both of the troublesome quantities—both the absolute value of the average and the absolute measure of variability. Let the average in each case be counted as 0, *i. e.*, let the individual's standing be expressed as a *deviation* above or below the average; and further, let the measure of variability be taken as the unit deviation, and all deviations be expressed as fractions or multiples of this unit. (For the measure of variability, either the average deviation, or the

[1] For the tests named in the table, see Woodworth and Wells, Association Tests, Psychological Review, Monogr. Suppl., 1911. The number of individuals tested in the above series, thirteen, was too small for final certainty.

mean square deviation, or the quartile, etc., may be chosen.) What this method does is to assign each individual's *position in the distribution* of the group: he stands, namely, above or below the group average, and so and so much above or below as compared with the average variation of the group.

No assumption is made by this method as to the ratio between the variability and the group average; for the average is taken as 0 and the variability as 1, independently the one of the other. The only assumptions underlying the method are those involved in every use of averages and variabilities, namely, that the average means the same thing in respect to one distribution as in respect to another, and, likewise, that the measure of variability means the same thing in respect to the different distributions. Both of the assumptions are correct if the distributions are of the 'normal' type, or if all the distributions belong to any one type. Were one distribution normal, another markedly skew, and a third distinctly bimodal, neither the average nor the average deviation would mean quite the same thing in respect to the three, and the method would be illegitimate; but in such a case it is doubtful if the distributions ought properly to be combined at all. Mental tests usually give group distributions not very different from the 'normal,' though tending on the whole to be somewhat skew in such a way that more individuals lie on the good side of the average than on the bad side. The distributions for different tests do not differ much in shape, and no considerable error can be introduced by placing the average always equal to 0 and the average deviation (or mean square deviation, etc.) always equal to 1.[1]

When the individual's position in each single distribution

[1] To repeat: this procedure involves no assumptions that are not involved in the ordinary statistical operations with averages, average deviations and coefficients of correlation. It is simply assumed to be fair to compare the individual with the group average as a standard, and to measure his deviation from the standard in terms of the group variability, and, further, to compare the results so obtained in different tests. If the average means anything that is constant for all the distributions, and if the average deviation means anything that is the same for all the distributions, then the assumptions are justified. It is true, of course, that the method always measures the individual by his relations to the group, and that, for some purposes, absolute and not relative measures are what is required.

has thus been determined, his *average position* in two or more distributions can be got, as well as the variability of his positions. If he stands + .8, + .4, and − .3 in three tests, his average standing is + .3, and the a.d. of his position is .4. If one asks, "Four-tenths of what?" the answer is that *the unit is throughout the variability of the group in the single test.*

The method and its utility will now be illustrated by the results of a set of association tests. Thirteen college and university students (eight men and five women) were examined with nine rather similar tests, called the 'logical relations' tests. The results of each single test were first treated by themselves: the average time of the thirteen subjects was found, and the + or − deviation of each individual. The mean square deviation was then found and used as the unit, all the single deviations being divided by it and expressed as per cents of it. The individuals who did better than the average were marked + and those who did worse than the average were marked −. In the accompanying table, each vertical column contains the standings of the same individual in the different tests. F_1, F_2, etc., are the women, and M_1, M_2, etc., the men. The individuals are arranged, from left to right, in the order of their average proficiency in these tests. Below the single records of each individual is a number in heavy type giving his average standing, and below this, again, is a measure of his variability of standing. (In this table, the mean square deviation, σ, has been consistently used as the measure of variability. The relative advantages of this measure and of the a.d. will be discussed later on.)

Inspection of the table shows at once that a high positive correlation is to be expected, and it further shows certain facts regarding the individuals which would be lost if only coefficients of correlation were computed. For example, the individual F_1 consistently occupies a very high position in all the tests, and the positive correlations are due to this individual's consistency more than to any other individual, though there are a few who are nearly as consistent in occupying a low position, and one or two who are fairly consistent in occupying a middle position. There are, on the other hand, individuals whose

STANDINGS OF 13 INDIVIDUALS IN THE LOGICAL RELATIONS TESTS

$$Unit = \frac{\sigma}{100}$$

Individuals.	F_1	M_1	F_2	M_2	M_3	M_4	F_3	M_5	M_6	F_4	F_5	M_7	M_8
Tests													
Opposites	+159	+ 58	+131	+117	+ 6	− 43	+ 37	+ 18	−117	−124	− 7	− 80	−177
Verb-object	+172	+ 71	+ 73	+ 41	+17	+ 46	+106	−124	− 95	− 52	− 97	−190	+ 28
Subord. concept	+189	+152	− 41	+ 21	− 8	+111	−115	− 4	+ 37	− 98	− 94	+ 8	−152
Supraord. concept	+197	+135	+ 94	− 4	+21	+ 8	− 16	+ 29	− 37	− 12	−172	−131	−131
Part-whole	+192	+ 20	+104	+ 44	+16	− 44	+ 12	+ 84	+ 36	− 84	− 92	− 76	−224
Whole-part	+148	+ 53	+ 95	+111	+ 5	− 21	+127	+ 27	− 48	−164	−143	−117	− 64
Agent-action	+192	+110	+ 89	+ 75	+ 7	− 62	−110	+ 55	+ 7	− 82	− 21	−185	− 82
Action-agent	+152	+ 72	+104	+ 32	+52	+ 24	− 52	− 60	− 56	+ 44	− 36	− 4	−268
Attribute-substance	+155	+125	+ 77	+ 5	+53	− 58	− 48	−259	− 43	− 24	+ 53	+ 38	− 55
Average standing in logical relations (s)	+173	+ 88	+ 81	+ 49	+19	− 4	− 7	− 26	− 35	− 66	− 68	− 82	−125
Variability of individual's standing in logical relations (σ_i)	18	42	45	44	20	53	81	101	50	60	67	78	87

standing varies much from test to test. This individual difference in consistency finds an expression in the variability of the individual's standing, σ_i, the mean square deviation of the individual's standing. In no other way, probably, except by the method here employed, would it be possible to reach a measure of this sort of personal characteristic, though it is a characteristic that impresses the experimenter as he conducts subjects through a series of tests. Of some individuals he would, after experience with them, be greatly surprised to see them do badly; of others he would be surprised to find them doing well; while of still others he would not be surprised at anything. These impressions of the experimenter are confirmed and made exact by the measure here given.

When the average standing is compared with the individual variability, it is seen that the individuals who stand high are more consistent than those who stand low. The Pearson coefficient of correlation between the standing and the consistency is .72. Some individuals with a middle average standing have, indeed, very high variabilities; and this would be expected, since any individual who does vary greatly from good to bad will tend toward a middle average position. It might be expected that the individuals with the lowest standing would be as consistent in their inferiority as the best individuals were in their superiority; but this is not true within the range of individuals here examined; for, whereas the best individuals suffered no lapse towards mediocrity, the worst individuals sometimes gave an unexpected exhibition of efficiency.

II

Since the labor involved in the above method is considerable, it may be well to show that, once this labor is performed, the operation of computing coefficients of correlation is considerably simplified; so that, if the investigator desires to measure the average correlation, he is compensated therein to some degree for the time spent in obtaining the average individual standings. In addition, he is likely to gain a more precise insight into the causes of correlation from being able to trace the individual contribution to the average result, and

from being able to obtain, besides the Pearson coefficient, certain other measures of correlation which are in some respects more easily understood.

What the writer here attempts is to derive *simplified formulæ*, available when the original measures have been reduced to terms of the a.d. or the mean square deviation, for (1) the Pearson coefficient of correlation between two tests; (2) the average correlation within any number of tests, as measured by Boas;[1] and (3) the Spearman correction for attenuation.

In the mathematical considerations which follow, I shall first assume that the original measures have been reduced to terms of the mean square deviation (σ), and later shall consider what modifications are required if the reduction has been to terms of the a.d.; and shall finally consider the relative advantages of the two reductions.

Consider first two tests, A and B, with each of which a measure is obtained of the individuals 1, 2, 3, ... Let the original measures be reduced, as above explained, to deviations divided by the mean square deviation in each test; and let a_1 be the reduced measure of individual 1 in test A, etc. Then the two series of measures may be represented thus:

$$a_1\ a_2\ a_3\ a_4\ a_5\ \ldots \quad \Sigma a = 0; \quad \text{Av}\, a^2 = 1$$
$$b_1\ b_2\ b_3\ b_4\ b_5\ \ldots \quad \Sigma b = 0; \quad \text{Av}\, b^2 = 1.$$

$\Sigma a = 0$; *i. e.*, the algebraic sum of the deviations is 0, because the sum of the positive deviations from the average = the sum of the negative deviations. Av $a^2 = 1$; *i. e.*, the mean square deviation = 1, being taken as the unit.[2]

Now let the average standing of each individual be found, and represented by $s_1, s_2, s_3, \ldots$, in which

$$s_1 = \frac{a_1 + b_1}{2}, \quad s_2 = \frac{a_2 + b_2}{2}, \text{ etc.}$$

[1] *Science*, 1909, **29**, 824.

[2] The use of the symbols Σ and Av in what follows will be sufficiently obvious. The main points are that they can be distributed to the several terms in a sum (*i. e.*, $\Sigma(a + b + c) = \Sigma a + \Sigma b + \Sigma c$, and $\text{Av}(a + b + c) = \text{Av}\, a + \text{Av}\, b + \text{Av}\, c$, and that a constant factor can be freely moved from one side to the other of the symbol (*i. e.*, if p is constant, $\Sigma pa = p\Sigma a$; and $\text{Av}\, pa = p\ \text{Av}\, a$; also, of course, $\text{Av}\, p = p$).

Then

$$\Sigma s = \Sigma \frac{a+b}{2}$$
$$= \tfrac{1}{2}(\Sigma a + \Sigma b)$$
$$= 0, \qquad (1)$$

since both Σa and Σb are 0. That is to say that the s's are deviations from the center of the average distribution; so that the variability of the s's, or σ_s, is given by the equation,

$$\sigma_s^2 = \text{Av } s^2. \qquad (2)$$

We wish now to find the coefficient of correlation between A and B. The well-known formula for the Pearson coefficient is

$$r = \frac{\Sigma ab}{n\sigma_a\sigma_b},$$

in which a and b are deviations. Here, however, both σ_a and σ_b have been taken as units, and therefore,

$$r = \frac{\Sigma ab}{n}$$
$$= \text{Av } ab. \qquad (3)$$

In words, the coefficient of correlation is the average product of the reduced measures of an individual.

Another expression for the coefficient has some advantages. It is obtained as follows:

$$\text{Av } s^2 = \text{Av}\left(\frac{a+b}{2}\right)^2$$
$$= \tfrac{1}{4} \text{ Av } (a+b)^2$$
$$= \tfrac{1}{4} \text{ Av } (a^2 + b^2 + 2ab)$$
$$= \tfrac{1}{4} (\text{Av } a^2 + \text{Av } b^2 + 2 \text{ Av } ab)$$
$$= \tfrac{1}{4}(1 + 1 + 2r)$$
$$= \tfrac{1}{2}(1 + r).$$

And therefore, by transposition,

$$r = 2 \text{ Av } s^2 - 1,$$

or,

$$r = 2\sigma_s^2 - 1. \qquad (4)$$

If the s's have already been found, in order to know each individual's average standing, then the r can be more easily obtained by this formula than from Av ab.

Consider next the case in which more than two tests are in question. As before, let the individuals be designated 1, 2, 3, . . . , the whole number being denoted by n; and let the tests be designated A, B, C, . . . , the number of tests being m. Then a table of the reduced results takes the form

$a_1 \quad a_2 \quad a_3 \quad a_4 \quad \ldots$	$\Sigma a = 0; \quad \text{Av } a^2 = 1$
$b_1 \quad b_2 \quad b_3 \quad b_4 \quad \ldots$	$\Sigma b = 0; \quad \text{Av } b^2 = 1$
$c_1 \quad c_2 \quad c_3 \quad c_4 \quad \ldots$	$\Sigma c = 0; \quad \text{Av } c^2 = 1$
. . . . .	. . . . .
. . . . .	. . . . .
$s_1 \quad s_2 \quad s_3 \quad s_4 \quad \ldots$	$\Sigma s = 0; \quad \sigma_s^2 = \text{Av } s^2$

$$s_1 = \frac{1}{m}(a_1 + b_1 + c_1 + \cdots), \text{ etc.},$$

from which it is evident, as above in the case of two tests, that $\Sigma s = 0$; and hence the s's are their own deviations, and

$$\sigma_s^2 = \text{Av } s^2. \tag{5}$$

To find a measure of the mutual agreement of all the tests, we may find the average of the coefficients of correlation between the pairs of tests. This can be got in terms of Av s^2, as follows:

$$\text{Av } s^2 = \text{Av}\left(\frac{a + b + c + \cdots}{m}\right)^2$$

$$= \frac{1}{m^2}\,\text{Av}\,(a + b + c + \cdots)^2$$

$$= \frac{1}{m^2}(\text{Av } a^2 + \text{Av } b^2 + \text{Av } c^2 + \cdots + 2\,\text{Av } ab + 2\,\text{Av } ac + 2\,\text{Av } bc + \cdots).$$

But this expression can be much simplified, for Av $a^2 = 1$, Av $b^2 = 1$, etc., there being m of these terms that reduce to 1. Moreover, Av $ab = r_{a,b}$, Av $ac = r_{a,c}$, etc.; and there are similar terms for all the r's. Making these substitutions, we have

$$\text{Av } s^2 = \frac{1}{m^2}[m + 2\Sigma r].$$

Now we desire the average r. The number of r's is equal to the number of ways in which m tests can be compared two at a time, or $\frac{m(m-1)}{2}$,

and therefore

$$\Sigma r = \frac{m(m-1)}{2} \text{ Av } r.$$

Making this substitution, we have

$$\text{Av } s^2 = \frac{1}{m^2}[m + m(m-1) \text{ Av } r]$$

$$= \frac{1}{m}[1 + (m-1) \text{ Av } r].$$

Therefore, finally,

$$\text{Av } r = \frac{m \text{ Av } s^2 - 1}{m - 1}, \tag{6}$$

or,

$$\text{Av } r = \frac{m\sigma_s^2 - 1}{m - 1}. \tag{7}$$

Once the average standings of the individuals are found, therefore, very little further work is necessary to calculate the average coefficient of correlation of the whole series of tests.

When $m = 2$, this expression reduces to that already given for the correlation between two series, viz.,

$$r = 2 \text{ Av } s^2 - 1 = 2\sigma_s^2 - 1.$$

Since Av r and Av s^2 are so simply related to each other, it is evident that either might stand as a measure of the agreement of the set of tests. It will be worth while to dwell on these two measures for a moment.

Since the maximum value of any r is 1, the maximum value of Av r is 1, and in this case the equation (6),

$$\text{Av } r = \frac{m \text{ Av } s^2 - 1}{m - 1},$$

shows that Av $s^2 = 1$. This then is the condition for perfect correlation between all the tests. That this is so is also evident directly, for perfect correlation means that each indi-

vidual has the same standing in any one test as he has in any other; and therefore his average standing, s, is the same as his standing in any test, such as a. Accordingly,

$$\text{Av } s^2 = \text{Av } a^2 = 1.$$

Or, otherwise expressed, when correlation is perfect,

$$\sigma_s = 1$$

or, the variability of the average distribution is equal to the variability of any single distribution. Anything less than perfect correlation causes the average distribution to fall together, and the variability of the average distribution to be less than 1, *i. e.*, less than that of the single distribution. The minimum value of Av s^2 (since the square of a real number cannot be negative) is 0; and Av s^2 could not be 0 unless each and every s were 0, which would mean that all the individuals were equal in the average distribution. This is obvious when the number of tests is only two, for then nothing would reduce all the individuals to equality in the average, except complete compensation or opposition between the results of the two tests. The formula shows the same, for, when $m = 2$, and Av $s^2 = 0$, $r = -1$. In general the minimum value of Av r is found by putting Av $s^2 = 0$ in the above equation, whence

$$\text{Av } r = -\frac{1}{m-1}.$$

When $m = 2$, this gives Av $r = -1$; when $m = 3$, Av $r = -\frac{1}{2}$, when $m = 4$, Av $r = -\frac{1}{3}$, etc., in a series of negative values tending toward 0 as the number of tests increases. This is vaguely obvious to common sense, for not more than two sets of measures can be in complete opposition, each to each.

In some respects, σ_s gives a more intelligible measure of the agreement of several tests than is afforded by the more familiar Av r. It shows how much the variability of the average distribution is lessened in comparison with the variability of the single distribution. It ranges between the extremes 1 and 0, no matter what the number of tests; 1

always signifying perfect agreement and 0 the most complete disagreement possible; whereas the lower limit of Av r rises with the number of tests. On the other hand, Av r has the advantage of always being 0 for a chance relation of the several tests, while the value of σ_s, corresponding to Av $r = 0$, changes with the number of tests. If Av r is made 0, the formula (7) shows that

$$\sigma_s^2 = \frac{1}{m},$$

or,

$$\sigma_s = \sqrt{\frac{1}{m}}$$

When $m = 2$, then,

$$\sigma_s = \sqrt{\frac{1}{2}} = .71;$$

when $m = 3$, $\sigma = .57$; when $m = 4$, $\sigma_s = .50$, etc., for chance correlation.

Still another measure of correlation deserves brief mention. Evidently, perfect correlation requires that each individual shall maintain always the same position in the several distributions; his variability of standing must be 0. As the individual's variability increases, the agreement between the tests decreases; and the maximum of disagreement would require that the individual's position in the different distributions should vary, on the average, as much as the positions of the different individuals in the single test.

An expression for Av σ_i^2 would thus be a measure of the average consistency of the individuals, and so of the correlation of all the tests.

Consider again the symbolic table

a_1	a_2	a_3	a_4	. . .
b_1	b_2	b_3	b_4	. . .
c_1	c_2	c_3	c_4	. . .
.	.	.	.	. . .
.	.	.	.	. . .
s_1	s_2	s_3	s_4	. . .
σ_1	σ_2	σ_3	σ_4	

The symbol σ_1 is used for the mean square deviation of individual 1 about his average standing s_1. His single deviations from this average are $s_1 - a_1$, $s_1 - b_1$, $s_1 - c_1$, . . . , and therefore,

$$\sigma_1^2 = \frac{1}{m}[(s_1 - a_1)^2 + (s_1 - b_1)^2 + (s_1 - c_1)^2 + \cdots]$$

$$= \frac{1}{m}[s_1^2 - 2s_1a_1 + a_1^2 + s_1^2 - 2s_1b_1 + b_1^2 + s_1^2 - 2s_1c_1 + c_1^2 + \cdots]$$

$$= \frac{1}{m}[ms_1^2 - 2s_1(a_1 + b_1 + c_1 + \cdots) + (a_1^2 + b_1^2 + c_1^2 + \cdots)]$$

$$= s_1^2 - 2s_1\frac{a_1 + b_1 + c_1 + \cdots}{m} + \frac{1}{m}(a_1^2 + b_1^2 + c_1^2 + \cdots)$$

$$= -s_1^2 + \frac{1}{m}(a_1^2 + b_1^2 + c_1^2 + \cdots),$$

since

$$\frac{a_1 + b_1 + c_1 + \cdots}{m} = s_1.$$

This gives the σ_i^2 for one individual; and if it is found for each of the n individuals, and the results are all added together and divided by n, we get

$$\text{Av } \sigma_i^2 = -\text{Av } s^2 + \frac{1}{mn}[a_1^2 + b_1^2 + c_1^2 + \cdots + a_2^2 + b_2^2 + c_2^2 + \cdots + a_3^2 + b_3^2 + c_3^2 + \cdots + \ldots]$$

$$= -\text{Av } s^2 + \frac{1}{mn}[\Sigma a^2 + \Sigma b^2 + \Sigma c^2 + \cdots]$$

$$= -\text{Av } s^2 + \frac{1}{m}[\text{Av } a^2 + \text{Av } b^2 + \text{Av } c^2 + \cdots]$$

$$= -\text{Av } s^2 + \frac{1}{m}[1 + 1 + 1 + \cdots]$$

$$= -\text{Av } s^2 + 1,$$

since there are m of the terms that reduce to 1. Thus,

$$\text{Av } \sigma_i^2 + \text{Av } s^2 = 1,$$

or,

$$\text{Av } \sigma_i^2 + \sigma_s^2 = 1. \tag{8}$$

This equation, along with the equation, Av $s = 0$, affords a good check on the accuracy of the computations.

The demonstration of the above equation can be put into a more compact form by employing the letter x to represent any one of the letters a, b, c, . . . , and by using Av_m to indicate the average of m quantities corresponding to the m tests, and Av_n to indicate the average of n quantities corresponding to the n individuals. Then, for any one individual, we have

$$\begin{aligned}\sigma_i^2 &= \text{Av}_m (s - x)^2 \\ &= \text{Av}_m s^2 - 2s \text{Av}_m x + \text{Av}_m x^2 \\ &= s^2 - 2s^2 + \text{Av}_m x^2 \\ &= - s^2 + \text{Av}_m x^2.\end{aligned}$$

And, for all the individuals, we have

$$\begin{aligned}\text{Av}_n \sigma_i^2 &= - \text{Av}_n s^2 + \text{Av}_{mn} x^2 \\ &= - \text{Av}_n s^2 + 1,\end{aligned}$$

as before.

A demonstration very similar to this last shows another interesting property of σ_s. Sometimes it is desired to correlate the standing of the individuals in a single test with their average standing in all the tests; for if the correlation is high, the single test is indicated as being a good measure of the whole set of similar performances. If the coefficient of correlation between a single test, A, and the average of all be designated by $r_{a,\,s}$, then

$$\begin{aligned}r_{a,\,s} &= \frac{\text{Av}_n as}{\sigma_a \sigma_s} \\ &= \frac{\text{Av}_n as}{\sigma_s},\end{aligned}$$

since $\sigma_a = 1$. If each of the m tests, A, B, C, . . . , be corre-

lated with the average of all, and if x be used to indicate any one of the letters a, b, c, . . . , then

$$\text{Av}_m\, r_{x,\,s} = \text{Av}_m\left(\frac{\text{Av}_n\, xs}{\sigma_s}\right)$$

$$= \frac{1}{\sigma_s}\,\text{Av}_{mn}\, xs$$

$$= \frac{1}{\sigma_s}\,\text{Av}_n\,\text{Av}_m\, xs$$

$$= \frac{1}{\sigma_s}\,\text{Av}_n\,(s\,\text{Av}_m\, x)$$

(since, for any column of m numbers, as a_1, b_1, c_1, . . . , the corresponding s_1 is constant)

$$= \frac{1}{\sigma_s}\,\text{Av}_n\, s^2$$

(since $\text{Av}_m\, x$ = the average of the m numbers in one column, as a_1, b_1, c_1, . . . , and the corresponding s_1 is this average)

$$= \frac{\sigma_s^2}{\sigma_s}$$

$$= \sigma_s. \qquad (9)$$

Therefore σ_s is the average value of the coefficients obtained by correlating the average standings with the standings in each single test.[1]

[1] It is instructive to apply such an equation to special cases. When the number of tests is 2, we have the equations, (4) and (9),

$$r_{x,\,y} = 2\sigma_s^2 - 1;$$
$$\text{Av}\, r_{x,\,s} = \sigma_s.$$

Whence

$$\text{Av}\, r_{x,\,s} = \sqrt{\frac{r_{x,\,y}+1}{2}}.$$

If, then, $r_{x,\,y} = 0$, *i. e.*, if the two tests have only a chance relation with each other, then the average standing in the two correlates with either one according to the coefficient,

$$r_{x,\,s} = \sqrt{1/2} = +.71.$$

This affords a rather tangible notion of the degree of correspondence indicated by the coefficient + .71. When two series of measurements give the coefficient + .71, the agreement between them is the same as that obtaining between a series of measurements and the average of itself with a totally unrelated series. Or, one might almost say, if two wholly independent factors contribute equally to a variable result, the result gives a coefficient of + .71 with that which would be given by either factor acting alone.

Now for the Spearman correction for attenuation. If A and B are two tests, presumed to be tests of the same function, and X and Y are two tests of another function, then Spearman gives the following formula for the true coefficient of correlation between the two functions:

$$r = \frac{\frac{1}{4}(r_{a,x} + r_{a,y} + r_{b,x} + r_{b,y})}{\sqrt{r_{a,b} \cdot r_{x,y}}}.$$

That is to say, we are required to find the Pearson coefficients between A and X, between A and Y, between B and X, and between B and Y; and also those between A and B and between X and Y; and then divide the arithmetical mean of the first four by the geometrical mean of the last two. This rather laborious process can be much abridged provided the results of each test have been for other reasons reduced to terms of σ, and if also the average position of each individual in the pairs of tests, A and B, and X and Y, has been determined. The symbol $s_{a,b}$ may be employed to denote the average standing of any individual in the two tests A and B. Spearman's formula reduces as follows:

$$\begin{aligned}
r &= \frac{\frac{1}{4}(r_{a,x} + r_{a,y} + r_{b,x} + r_{b,y})}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\
&= \frac{\frac{1}{4}(\mathrm{Av}\,ax + \mathrm{Av}\,ay + \mathrm{Av}\,bx + \mathrm{Av}\,by)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\
&= \frac{\frac{1}{4}\mathrm{Av}(ax + ay + bx + by)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\
&= \frac{\frac{1}{4}\mathrm{Av}(a + b)(x + y)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\
&= \frac{\mathrm{Av}\left(\frac{a+b}{2}\right)\left(\frac{x+y}{2}\right)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\
&= \frac{\mathrm{Av}\, s_{a,b}\, s_{x,y}}{\sqrt{r_{a,b} \cdot r_{x,y}}}. \qquad (10)
\end{aligned}$$

We have therefore to proceed as follows: Find the standing of each individual (in terms of σ) in test A and in test B,

and his average standing in these two, and also the correlation between these two; likewise his standing in X, in Y, and his average standing in these two, as well as the correlation between them. Then multiply each individual's average standing in A and B by his average standing in X and Y, and divide as indicated in the formula.

The preceding formulæ have been based on the reduction of the original measures to terms of σ, and it remains to be seen whether the formulæ can be adapted to use when measurements are reduced to terms of a.d. This can easily be accomplished on the assumption of a constant relation between σ and a.d. Theoretically, $\sigma = 1.2533$ a.d., *i. e.*, this is the ratio between the two measures in the normal distribution. This ratio will seldom be found exactly realized when a.d. and σ are calculated from short series of measurements; partly because the distributions are not strictly normal, and partly because neither σ nor a.d., as calculated from a short series of measurements, exactly represents what would be the σ or a.d. if the number of measurements were sufficiently large. As between these two measures of variability, preference is given by statisticians to σ, because its true value is usually more closely approximated in a small series of measurements. On the other hand, psychologists have often expressed a preference for a.d., because it is less affected by the extreme accidental variations that frequently occur in mental performances. In mental tests, a.d. is subject to frequent small errors, and σ to occasional large errors.

When expressed in terms of a.d., each individual's standing will evidently have the same sign as when expressed in terms of σ, and will simply be numerically larger in the ratio of σ to a.d. Each individual's standing will therefore be expressed by a number 1.2533 times as large (theoretically) as when σ is used as unit. (Or, if any other measure of variability, such as the quartile, were used as the unit, the reduced measures in any series would have a constant ratio to those obtained with σ as unit.) If, for convenience, the letter p be used to represent the ratio of σ to a.d. (or other unit

employed), and if a_1, a_2, a_3, . . . and b_1, b_2, b_3, . . . be used, as before, to designate the deviations expressed in terms of σ, then pa_1, pa_2, pa_3, . . . , and pb_1, pb_2, pb_3, . . . will be the measures expressed in terms of a.d.

Now $$r_{a,b} = \text{Av } ab$$

$$= \frac{1}{p^2} \text{Av } (pa)(pb).$$

When, therefore, the unit in each series is the a.d., the coefficient of correlation is found by taking the average product of an individual's marks, and dividing by $(1.2533)^2$, or, what is the same thing, multiplying by 0.64 (two places being all the accuracy the whole method can stand).

This way of finding r is not, however, to be recommended, for the reason that, when p is actually different from the theoretical value, the value of r does not have its accustomed limits. Thus, if p equals, in each of two given series of measurements, 1.10 instead of 1.25, the maximum value of r, for perfect correlation, is $+ .78$ instead of the customary $+ 1.00$; and if $p = 1.40$, the maximum value of $r = + 1.26$. Since the utility of r is dependent on the constancy of its limits, such irregularities would spoil it.

The following considerations do not depend on the absolute value of the ratio between σ and a.d., though they do assume the *constancy* of the ratio. After the formulæ have been derived, consideration will be given to the inaccuracy introduced when the ratio is not constant. The ratio is still represented by p.

Let individuals 1, 2, 3, . . . be measured by tests A, B, C, . . . , and let the results be reduced, first to terms of σ, and again to terms of a.d., and let tables prepared by the two methods be set down side by side; thus, symbolically:

In terms of σ	In terms of a.d.
a_1 a_2 a_3 a_4 . . .	pa_1 pa_2 pa_3 pa_4 . . .
b_1 b_2 b_3 b_4 . . .	pb_1 pb_2 pb_3 pb_4 . . .
c_1 c_2 c_3 c_4 . . .	pc_1 pc_2 pc_3 pc_4 . . .
. . . . .	. . . . .
. . . . .	. . . . .
s_1 s_2 s_3 s_4 . . .	ps_1 ps_2 ps_3 ps_4 . . .

Since, as previously shown, $\Sigma s = 0$,

$$\Sigma ps = p\Sigma s = 0, \tag{11}$$

and therefore, ps_1, ps_2, ps_3, . . . are deviations from the center of the average distribution. Accordingly the numerical average of these deviations is the a.d. of the average distribution, or a.d._{ps}. Now

$$\text{a.d.}_{ps} = p \times \text{a.d.}_s$$

But, since the ratio of a.d. to σ is (theoretically) still the same in this average distribution as in any other,

$$\text{a.d.}_s = \frac{1}{p}\sigma_s,$$

and, therefore,

$$\text{a.d.}_{ps} = p \times \text{a.d.}_s = p \times \frac{1}{p}\sigma_s = \sigma_s. \tag{12}$$

In other words, if, in one case, the σ of the single distribution is taken as unit, and in another case the a.d. of the single distribution is taken as unit, the numerical values of σ of the average distribution in the first case and of a.d. of the average distribution in the second case will be the same. Or, again, a.d. of the average distribution has the same ratio to a.d. of the single distribution as σ of the average distribution has to σ of the single distribution. This seems rather obvious, but, as will be shown later, it is not always true, and some difficulty in the use of the following formulæ may arise when it is not true.

Since, then, $\text{a.d}_{ps}, = \sigma_s$, we can substitute in the previous formula (7) for the average coefficient of correlation, and have, instead of

$$\text{Av } r = \frac{m\sigma_s^2 - 1}{m - 1},$$

the following formula, to be used when the data of each test are expressed in terms of a.d. as unit:

$$\text{Av } r = \frac{m(\text{a.d.}_{ps})^2 - 1}{m - 1}.$$

The p in the subscript of a.d._{ps}, being of use only to distinguish the procedure with a.d. as the unit from the procedure with

σ as the unit, can be dropped from the final formula, with the understanding that the formu'a is only to be used when a.d. is the unit throughout; then we have

$$\text{Av } r = \frac{m \text{ a.d.}_s^2 - 1}{m - 1}. \qquad (13)$$

What was said before of the use of σ_s as a measure of correlation evidently applies also to a.d.$_s$. The value of a.d.$_s$ is 1 for perfect agreement between all the tests, and 0 for the most complete disagreement possible.

When the number of tests, or m, is 2, the above formula reduces to the following:

$$r = 2 \text{ a.d.}_s^2 - 1. \qquad (14)$$

This gives a quick way of computing r, and is worthy to be regarded as one of the best abridged or 'foot-rule' methods of finding the Pearson coefficient. Its use may be illustrated in an actual case.

When the results of two color naming tests on 13 individuals are expressed in terms of a.d., they appear as in the accompanying table. The line beginning s gives each individual's average standing, and the numerical average of the s's gives a.d.$_s$. Here a.d.$_s$ = .96, and the formula,

$$r = 2 \text{ a.d.}_s^2 - 1,$$

gives $r = +.84$. Calculated from the regular Pearson formula, $r = +.83$. Such close agreement as this is not always to be expected, but in my experience the agreement is usually close.[1]

[1] This method can be still further abridged, for since the average of the numbers in the first line = 1 (disregarding signs), and likewise the average of the numbers in the second line = 1, the average of them all (still disregarding signs) must = 1. Now this average of all would be a.d.$_s$, except for the fact that in finding each s the signs are considered. If therefore the signs had been disregarded in finding the s's, a.d.$_s$ would have been = 1. It is only the cases of unlike signs that diminish the value of a.d.$_s$. In the table, M_2 and M_6 are the only cases that cause of diminution of a.d.$_s$. If, in these two cases, the arithmetical sums of 29 and 75, and of 24 and 75, had been taken, instead of the arithmetical differences, a.d.$_s$ would have been equal to 1. Take the case of M_2. By taking the difference of 29 and 75, instead of their sum, the corresponding s is made less by 29 than it would have been; and, in any case, the value of s is diminished by the amount of the smaller of the two numbers which have unlike

RESULTS OF THE COLOR-NAMING TESTS, EXPRESSED IN TERMS OF A.D.

Individuals	F_1	M_1	F_2	M_2	M_3	M_4
Test 1	+187	+108	+160	+29	−116	+ 3
Test 2	+ 85	+ 57	+160	−75	−123	+113
s	+136	+ 82	+160	−23	−119	+ 58

Individuals	F_3	M_5	M_6	F_4	F_5	M_7	M_8
Test 1	+160	− 24	−24	+3	−155	−24	−313
Test 2	+151	−113	+75	0	− 19	−38	−283
s	+155	− 68	+25	+1	− 87	−31	−298

That other measure of correlation, the Av σ_i^2 is also available when a.d. has been chosen as unit. For consider the measures of any one individual in the two tables above. When σ is the unit, his measures are $a_1, b_1, c_1, \cdots$; and, when a.d. is the unit, they are $pa_1, pb_1, pc_1, \cdots$. Then,

a.d. of $(pa_1, pb_1, pc_1, \cdots) = p \times$ a.d. of $(a_1, b_1, c_1, \cdots)$.

But

$$\text{a.d. of } (a_1, b_1, c_1, \cdots) = \frac{1}{p} \times \sigma \text{ of } (a_1, b_1, c_1, \cdots).$$

And therefore

$$\text{a.d. of } (pa_1, pb_1, pc_1, \cdots) = \sigma \text{ of } (a_1, b_1, c_1, \cdots).$$

In other words, the individual's variability of standing has the same numerical measure, whether σ or a.d. is used, consistently,

signs. Therefore, to get a.d.$_s$, we need only consider the cases of unlike signs; taking, in each such case, the smaller of the two numbers, finding the sum of these smaller numbers, and dividing by the whole number of individuals measured, we shall obtain the amount by which a.d.$_s$ is less than 1. Thus, in the table, taking the smaller of the two numbers under M_2 and M_8, we have, as their sum, 53, and dividing this by 13, the number of individuals, we have 4. Subtracting this from 1 (or 100 per cent.), we have a.d.$_s$ = .96. Now if all that were desired from the results were the coefficient of correlation, we should not need to reduce every deviation to terms of a.d., but we might simply do this in the cases of unlike signs, and then deal with the smaller of the resulting numbers as above. When the correlation is fairly high, so that the number of unlike signs is small, this method of computing r is certainly to be called a quick method. Certain inaccuracies that may creep in will be discussed later, but it may be noted at once that, unless there are unlike signs, the formula gives $r = 1$, though the correlation may evidently be not quite perfect, and though the formulæ in terms of σ may give a coefficient as low as + .80. Any rearrangement of the numbers in the two lines of the table, provided such rearrangement did not increase the number of unlike signed pairs, would not change the coefficient of correlations. From this it can be seen that the method lacks something in delicacy, but it is only in rather exceptional cases that this lack of delicacy actually changes the value of r.

as the measure of variability.[1] Accordingly, in the previous equation (8),

$$\text{Av } \sigma_i^2 + \sigma_s^2 = 1,$$

we may substitute a.d.$_i$ for σ_i, as well as (see above) a.d.$_s$ for σ_s, and obtain the following equation for the case when a.d. is used throughout instead of σ:

$$\text{Av (a.d.}_i)^2 + (\text{a.d.}_s)^2 = 1. \tag{15}$$

In like manner, the formula given above for the Spearman corrected coefficient of correlation can be used when a.d. has been taken as the unit, just as if σ were the unit. If we take the formula,

$$\text{Spearman } r = \frac{\text{Av } (s_{a,b}s_{x,y})}{\sqrt{\text{Av } ab \cdot \text{Av } xy}},$$

and simply substitute the values for a, b, x, y, s_{ab}, and s_{xy}, which are got (see p. ——) when a.d. is used as the unit, we have

$$\begin{aligned} &\frac{\text{Av } (ps_{a,b} \cdot ps_{x,y})}{\sqrt{\text{Av } (pa \cdot pb) \cdot \text{Av } (px \cdot py)}} \\ &\quad = \frac{p^2 \text{Av } (s_{a,b}s_{x,y})}{\sqrt{p^2 \text{Av } ab \cdot p^2 \text{Av } xy}} \\ &\quad = \frac{\text{Av } (s_{a,b}s_{x,y})}{\sqrt{\text{Av } ab \cdot \text{Av } xy}} \\ &\quad = r. \end{aligned} \tag{16}$$

To summarize: the formulæ developed for measuring correlation when the original measurements have been reduced to terms of σ can also be used when the measurements have been reduced to terms of a.d. One need only substitute systematically a.d. wherever σ appears. The assumption underlying the formulæ in terms of a.d. is the constancy of the ratio, p, between σ and a.d. Note that the absolute value of p is not in question, but only its constancy. It remains to consider how far the formulæ are vitiated by the actual variability of p.

[1] The theoretical relation between a.d. and σ would not be expected to hold except where the number of cases (here the number of tests) was considerable. Only then should the above equation for a.d.$_i$ be used.

There are two ways in which error might creep in through inconstancy of p.

1. The value of p may change from one test to another, and, in fact, does vary, in short series of measurements, from about 1.10 to about 1.40. This might conceivably cause trouble by so affecting the relative values of the s's that their distribution would be different according as σ or a.d. were used as the unit. When p varies from test to test, the values of s obtained in terms of a.d. need not be precisely proportional to those obtained in terms of σ. In practice, however, this source of error is negligible, first because the divergencies between the distribution of the s's is always slight, and second because, where divergencies exist, the a.d. values *may* be the truer. Examination of the derivation of formula (16) shows that the only p's there in question are those of the single tests and those expressing the ratio of the average standings, when a.d. has been the unit, to the average standings when σ has been the unit. Accordingly, this formula is not subject to any considerable error.

2. A more serious difficulty arises from the fact that p has been assumed to be constant, not only from one series of measurements to another, but also in the distribution of the average standings. It has been assumed, throughout the preceding deductions, that the ratio of σ to a.d. is the same in this average distribution as in the single distributions from which the average distribution is derived. Whether this is so or not depends on the nature of the correlation between the several series of measurements; and consideration of this matter brings to light an important point regarding correlations.

It has been shown above that the variability of the average distribution is less than that of the constituent single distributions, except when the correlation between the latter is perfect. Except in case of perfect positive correlation, σ_s is less than the σ of a single distribution (*i. e.*, less than 1, when σ of the single distribution is the unit), and $a.d._s$ is less than a.d. of the single distribution. In other words, except in case of perfect positive correlation, individuals differ less from one another on the average of several tests than in any single test.

Now if this diminution of the variability affected all parts of the distribution alike, so that the average distribution had the same form as the single distributions, then the ratio of σ to a.d., depending as it does simply on the form of the distribution, would be the same in the average distribution as in the single tests. (Or, if p varied from one test to another, it should have, in the average distribution, a value not far from the average of its values in the single tests.) But it may happen, at least in short series of measurements, that the diminution of variability, on taking the average of the tests, does not affect all parts of the distribution equally. The writer has observed, in practice, two opposed cases:

(*a*) The individuals lying near the extremes of the distribution maintain their positions from one test to another, whereas the individuals near the center of the distribution shift about considerably from test to test. The average distribution then presents a few excessively large deviations along with many small ones; and its σ is accordingly very large in relation to its a.d. In other words, p in the average distribution is larger than in the constituent distributions. Of the two p's which were assumed to cancel each other in deriving equation (12), that in the denominator is, under the present circumstances, larger than that in the numerator, and accordingly a.d.$_{ps}$ is actually smaller than σ_s; and hence the coefficient of correlation, computed from formula (13) or (14), comes out smaller than from formula (7) or (4), or from the regular Pearson formula.[1]

(*b*) The second case is the reverse of the first. The extreme individuals shift their positions considerably, while the middle individuals are more nearly constant. The average distribution then has many deviations of moderate size, and the ratio of σ to a.d. is small.[2] The use of a.d. then gives higher values for r than the use of σ.

[1] In two 'substitution tests,' I found $p = 1.18$ in the first test and 1.19 in the second, while it was 1.33 in the average distribution. Here r, by the Pearson formula or by formula (4), was $= + .42$, but by formula (14) it was $+ .11$.

[2] For example, in one 'form-naming test,' p was 1.25, and in another it was 1.12; in the average standings, it was 1.12. Here r, from the a.d. formula, was $+1.00$; but from the σ formula it was $+.79$.

In all such cases of a difference of result, according as σ or a.d. is the basis of calculation, the question remains open which result is the truer. Working with σ, as well as working with the Pearson formula, weights the extreme deviations rather heavily, and since extreme deviations are, in mental tests, often accidental, the weight thus given them may easily be excessive.

When a.d. has been used as the basis, and suspicion arises that the result would have been different if σ had been used, the value of σ_s can be approximately found, and all the values dependent on σ_s, without the need of going back over the whole work. If p_a, p_b, etc., be the ratios in the single tests, and p_m the average of these ratios, and if p_s be the ratio in the average distribution, then equation (12) may be re-written as follows:

$$\text{a.d.}_{ps} = \frac{p_m}{p_s}\sigma_s; \quad \text{or,} \quad \sigma_s = \frac{p_s}{p_m}\text{a.d.}_{ps}. \tag{17}$$

Now the ratio of σ to a.d. can be got from the reduced measurements for each test by simply computing the σ of the reduced measurements (the a.d. of these measurements being 1, since it was the unit of reduction), and p_s can be found from the a.d. and σ of the s's. In a similar way, equation (17) enables one, after conducting the work in terms of σ, to find also, approximately, the result which would have been obtained in terms of a.d.

In conclusion, it should be said that the formulæ developed in this paper, in so far as they depend on σ, are not to be regarded as approximations, for they are mathematically equivalent to the usual formulæ. The formulæ developed for use with a.d. are approximations, if the others are regarded as the norms; but where the results of the two general methods differ, it is by no means certain, *a priori*, which gives the truer result.

KNOWING SELVES[1]

BY JOHN E. BOODIN
University of Kansas

I

In trying to know the self, we must recognize in the first place that our concern must be with the finite self and its processes. We cannot even conjecture a mind different from ours. Such a mind must turn out in the last analysis to be an abstraction from our own experience. The idealistic absolute is merely our own ideal of a completed knowledge, not a different mind.

In the second place, the method pursued must be naturalistic. We must strive to know a self as we try to define a chemical element—through its conduct, not through *a priori* considerations. We would not say that the self is its behavior, any more than we would say that a chemical element is its behavior. It is not only the way it behaves, but the way it *can* behave in all possible situations. The self is what it must be taken as in its behavior, by itself and by others, in various contexts, physical and social, especially the latter. It is not something over and above the properties as known in situations; the essence appears completely, given the proper conditions. There is no substance except energy. The self can be as truly known as a chemical element in the tests of various situations. It has its breaking point in the stresses and strains of experience as surely as cast iron; its melting point as surely as gold; its freezing point as surely as water; its explosion point as surely as dynamite; its point of confluence with other selves as surely as wine mixes with water; it separates from other selves as surely as oil refuses

[1] The doctrine of "pragmatic realism" has been defined in its general outlines in "Truth and Reality," Macmillan, 1911. Two other concrete studies in this series are "Knowing Things," *Phil. Rev.*, July, 1911, and "Do Things Exist?" *Jour. Phil., Psych. and Sci. Meth.*, Jan. 4, 1912.

to mix with alcohol. Habits, motives, characters are but expectancies of varying complexity, which we can have as regards the self with reference to definite situations. Of these situations, the social situations are by far the most important —the only ones in fact which would make self-consciouness in the first instance possible. But secondarily at least physical situations, too, count. In them we learn our strength and courage and many other properties. The self in any case is what we must take it as being in conduct. It has spontaneity, if we must acknowledge spontaneity. It is a mechanism, just in so far as we can treat it that way. We must learn to take the mind *as known*, and not as the epiphenomenon of material processes on the one hand or of a transcendent substance on the other. We must start with facts, not with dogmas. Its properties indeed are different from those of material things. It has no mass or weight. But neither has electricity. Its properties differ in different situations. But so do those of any physical thing. The visual properties of the diamond don't cut glass. While difficult sometimes to calculate, owing to lack of organization or owing to complexity of motives, still its conduct is largely predictable. Human institutions of credit and confidence are built on such predictability. Taken in the average such predictability becomes well nigh absolute. Where the self differs radically from a physical thing is in the fact that consciousness is superadded to its activities and so gives them meaning and value for the self. But while this adds subjective significance, it does not prevent us from taking account of the properties of the self, past and present. And the property to have mercy is just as much of a property as the solidity of steel.

Like radium, mind is not as yet known to exist in an isolated state. We know mind, for certain at least, only in connection with physiological processes though we may hope for more corroborative evidence of the existence of mind after death. But while mind exists in connection with physiological processes, we know it nevertheless as pure; and we know it better than we know anything else. When

we take account of our own meaning or try to understand another living mind or try to get the significance of a poem, in either case, nerves don't get mixed up with ideas, any more than the letters on the page get confused with the meaning we try to decipher. We know mind as it is. Whether we know its existence apart from certain physiological conditions or not, itself we know as clear and distinct, a fact of its own kind, with its definite internal as well as external relations.

II

This is as true in knowing other minds as in knowing our own. The knowledge of other selves has been confused by two theories. One is the theory of analogy, viz., that we know other selves only by analogical inference, based upon the similarity of other bodies and their behavior with our own, while it is only our own mind that we know immediately. This theory confuses the problem of causality, with the problem of knowledge. It is true that our minds must make differences to our own bodies and their physical environment before their behavior can be overt to others and *vice versa*. But it is not true ordinarily that in knowing we argue back from bodily structure to mind. Man had composed great epics, laws and religions, built all the fundamental social institutions, before he knew there was such a thing as a nervous system. And even now the knowledge of the relation of mind and body is decidedly problematic and not to be compared with our knowledge of the mind's own relations, as we know it in logic, psychology and ethics. To be sure we sometimes start from structure in dealing with lower animal minds, but this is just the beginning of hypothesis, not real inference as to the mind's own nature. This must be understood through conduct—its intelligence and docility, quite independent of the presence of a nervous system. It certainly seems absurd to suppose that men should first study the connection of mental states and bodily expression in themselves and then read a mind back of the expression and structure of others—and that before they know anything about the connection of mind and body in themselves or

have even distinguished mind from body. It seems pretty clear that they start the other way; that they first learn to connect mind and conduct in others, before they become aware of the relation of mind and conduct in themselves. They learn to associate emotions and attitudes with expression in others before they are conscious of expression in themselves.

The other theory is the mystical theory. It argues for the immediacy of the knowledge of other minds without reference to expression. We immediately acknowledge other selves and that is all. Such acknowledgment is based upon no inference, implicit or explicit. It permits of no genetic analysis. Now this theory is certainly nearer true than the previous, from which it is a reaction. The knowledge of other selves may be regarded as immediate as that of our own. We know ourselves, as we know others, through the situations upon which we react. But this is quite different from holding that we have a mystic knowledge of ourselves in the abstract or of others in the abstract. In the abstract our significance equals zero. The knowledge of other selves is neither matter of analogical inference nor mystical appreciation but the homely way of reading conduct. And as social adjustment is a centrifugal process, it is natural that we should have formulated our own significance in terms of social situations —of social approval and disapproval, before we began to formulate the relation of social situations to our own ideals. The learning process is at first a purely objective process. A boy friend of three was confronted with a small misdemeanor. He recognized by the situation that it was a wrongness. He steadily maintained that he had not done it. His father sternly and sadly said, "Bobby, are you telling me a lie?" He was finally brought round to the right point of view with his mother's assistance and owned his act, with the solemn impression that the serious thing about it was telling a lie. The next day he astonished his parents by adding to an answer which he made "and it is not a lie." Through one tragic social context he had learned the significance not only of a word but of a social relation. It is

safe to say that he did not compare the parent's bodily expression with his own.

In the progress of experience, language as an artificial expression of mind, with its complex network of relations, largely takes the place of concrete situations for knowing minds. And our knowing our own mind, past and present, as well as knowing other minds, becomes the immediate recognition of the meaning of the language situations, until in the technical disciplines concrete imagery very largely drops out in our reading of meanings. The matrix of language, with its artificial equivalents for things and relations, becomes the social correlate of our communication and understanding of minds, not brain cells and association fibers. And logic, geometry and ethics as sciences of social mind relations reached a high perfection as sciences before neural physiology was born. In social communication, what we are immediately concerned with is words, conduct—not brain states. In talking with an individual, as in reading a book, we are concerned not with causes and effects—the producing of the spoken or written symbols and their reaching our, or the other party's sensorium. We are concerned with the interpretation of the symbols. The words are immediately associated with certain meanings; and our attention is fixed on the meanings, not on the instruments. As the ivy clings to its material framework which supports it, so do our meanings in every joint cling to language, only the meanings make their own framework as the nautilus builds its chambers.

While it is true that in understanding other selves we are dealing with the social matrix of language and meanings, still this does not prove that brain processes, nerves, vocal chords, air-waves and ears or eyes do not mediate causally between selves in communicating with each other. The teleological explanation and immediate acknowledgment of meaning by meaning would not be possible, in our sense world at least, if the communicating minds were not part of the causal nexus of the intervening world.

III

A great deal of mystery has been thrown about the dual nature of the self by traditional psychological theory. It is supposed that there is an absolute and constant distinction between the *knower* and the *known* or, to use James's phraseology, the *I* and the *me*. This is true not only of the old rationalistic psychology with its metaphysical soul, but it is true of recent treatments. Says Wundt: "Every experience contains two inseparable factors—objects of experience and the experiencing subject." [1] And Ebbinghaus: "Wherever thoughts and sensations are experienced, this subjective bearer to which they adhere, also becomes directly conscious in them and through them, in the same way as they themselves." [2] And even James: "It is obvious that if things are to be thought in relation, they must be thought together or in one something, be that something ego, psychosis, state of consciousness, or whatever you please." [3] This something to be sure is a "a spiritual something." Still its absolute distinction from the known content is implied. When I try to make clear to myself what this simple bearer is which is constant in all the states and logically distinct from them, it seems to be nothing else than the abstract fact of consciousness itself. This certainly is constant and simple and accompanies all our conscious states. It is also separable from them, as mind stuff need not be always conscious.

If what is meant is that all experience involves the subject-object relation or is representative, certainly some doubt may be thrown from the side of the facts. When we think, we of course always presuppose the subject-object relation, but is this true also of the simpler perceptual stage of experience? Could a creature depending upon impressions and upon learning by habit, without any images, say I? This does not seem likely, because there is no conscious context, which assimilates. In all experience too, there must be a beginning, a bare "awareness of" without any

[1] Wundt, *Definition der Psychologie*, Philosophische Studien, 1895.
[2] Ebbinghaus, *Grundzüge*, Vol. 1, p. 10.
[3] *Principles of Psychology*, Vol. 2, page 277.

"knowledge about," *i. e.*, without any associative context to react, where our experience *is* the light or the pain rather than *has* it. I do not see how such experience could have the "two inseparable factors, objects of experience and experiencing subject." In my own experience in waking up gradually after having been struck by lightning and snowed under in a storm on Gray's Peak, I could remember afterwards when I was a mass of pain and discomfort with no associations suggested in the way of danger or death. I could remember having seen the form of a man moving down the mountain side. But it was not until some time afterward that the perceptual picture suggested man and a futile cry for help and not till long afterwards that the perception suggested my companion and that the scene itself came back to me. There was certainly a period there of pure perception, while the associative context was as paralyzed as my bodily movements. I believe, in other words, that there is a simpler state of consciousness than the *I* and *me* relation—the state of bare awareness, which, of course, is not broken up except by a later more complex consciousness, which reflects upon it. Leaving aside, however, the question of the universality of the subject-object relation, what does it mean when we do have it? And is it such a mystery?

The mystery and the absoluteness and the constancy of the subject-object relations seems to disappear when we bring a little psychological analysis to bear. Abstracting from consciousness as bare awareness, we must make clear to ourselves what we mean by the subject-object relation in the concrete; and then we shall see that it is an associative context assimilating a selected content—the datum. The quality of *myness* is a function of the datum-being-selected by this individual interest. In other words, to say this is *my* object of consciousness and to say I am interested in this object are two different ways of saying the same thing. The I or subject in this relation is the active associative context, which we call interest, solicited by or striving to find its object, the me. In order to get rid of ages of false association we may call this context the *referent* and the datum, which is

selected, the *referatum*. Now my contention is that there is no absolute or constant relation between the selective context, the *referent*, and the selected object, the *referatum*. On the contrary, the distinction is relative to point of view and relative to time. In the first place, it is relative to point of view. This may be true within the same physical individual, as in the case of the divided self. In deliberation, the point of view shifts while the systems seem exclusive and constant. Now one system is tried out with reference to its antagonistic systems. And again the activity shifts to another system with its scale of values. But the struggle is precisely between coexisting and conflicting points of view. Here of course there is some common and constant group of tendencies which figures in the various systems and accounts for the shifting of attention. It is this common group of more or less implicit tendencies which gives rise to the feeling of outside push as regards the process of deliberation. The systems in intense moral struggle may coexist antagonistically for some time—each with a strong individual consciousness, and each struggling for the place of mastery, now one, now the other occupying the focal place—the lower taking the higher captive, the higher in turn summoning its energies against the lower, each very much alive and struggling for existence. In insanity we have cases of actual disruption of the various systems, when a man feels himself to be not one but many—thoroughly bewildered in the shifting and many-headed focus, as to which is I rather than the other. In every case of social communication where one system of meanings strives to understand another, the relation of *referent* and *referatum* is reciprocal and a matter of point of view.

Not only is the relation of I and me relative as between *coexisting* systems, internal or social, with their respective points of view. But the *same* system may now be *referent* and now *referatum* in the one personal history. In recalling a forgotten name, we use the meaning to find the name. The system of associations with its leading is the *referent*, the name the *referatum*. But having gotten the name, we

reverse the process and use the name with its larger context to fix the meaning. It is my purpose to open a refractory door. This system of tendencies, the *referent*, hunts about for means, the *referatum*. But in trying to solve the door situation, the purpose becomes aware of its own vagueness and limitations. It thus reverses itself in a measure; the door-consciousness with its associations defines the purpose to open and both are taken up in the larger context which was implicit in the procedure—getting what I wanted in the room, etc. The relation of I and me then is not a constant or absolute relation. There is no more mystery about the I than about the me. They continually shift places and *referent* becomes *referatum* and *vice versa*. They are both functions of a more or less definite system of tendencies which strives to realize itself and which we may call the self in the inclusive sense.

If this theory is true it should follow as a corollary, that self-consciousness, *i. e.*, consciousness of the I and me type, should be prominent in proportion to the activity of attention, being particularly obtrusive in the moments of embarrassment and frustration, while approaching the vanishing point with the fluency of the on-going of consciousness, when the felt unity radiates in all directions of the prevailing purpose. This seems actually carried out by the facts of experience. It is when the developing purpose is brought to halt, is balked for the time being, that the consciousness of meaning and datum, *referent* and *referatum*, becomes painfully strong. On the other hand when the flow is uninterrupted, when the purpose is absorbed in the transitions from phase to phase, whether the fascination be intellectual, practical or esthetic, the dualism of I and me approaches its vanishing point until lost in the mystic trance—the passive, coalescent state of attention.

Hume in speaking of the self as a "bundle of perceptions" fails to take account of the active dual character of the self. The self, whenever we are awake, is an *active* bundle of more or less systematized tendencies striving to appropriate or adapt itself to any external context, the datum. It is the latter context that has the stubborn perceptual character.

The self is not a bundle of perceptions. It is a bundle of tendencies, leadings, purposive striving. The perceptions, whether internal or external, are the facts taken account of —the me. You can't have an outside without an inside; and Hume[1] made the self all outside.

IV

Again, when we come to the identity of the self, we must hold that the self, like physical things, is just as constant as we can take it—as constant as its activities and contents; as its ability to satisfy social expectancies. What is the use of assuming, beside this constancy of the stream of consciousness, a substance to make the process constant? Such a substance is obviously an afterthought, an hypostasis of the fact of constancy itself. It makes the processes neither more nor less constant than they actually are in the flow of experience. As the organic sensations constitute a constant background in the changes of mental life, they play an important part in our consciousness of identity. They furnish largely the warmth and tone of personality; and disorganization of these sensations produces serious disturbances in our sense of self; yet to furnish the meaning of identity, there must also be certain constant tendencies, which furnish the leading or active thread of experience in the panoramic and shifting scenes of feeling, perceptions and ideas.

Kant is quite right that the consciousness of succession is a different fact from successive states of consciousness. But it does not follow that the consciousness of succession requires any transcendental unity outside of the stream of experience. The consciousness of succession means that a relatively permanent system of tendency, in order to realize its will, must take account of the coming and going of contents—must emphasize the constant as over against the fleeting in order to establish definite expectancies. What furnishes the pragmatic substance is precisely this core of permanent tendency and associations in the midst of the flux. What is the use of duplicating this identity by assuming an-

[1] *Treatise on Human Nature*, Vol. I., Part IV., 6.

other identity to account for it and so on *ad infinitum?* Why not take the constancy of the processes, so far as such constancy must be acknowledged, at its face value?

Constancy and change are both facts that the will must acknowledge and meet in the process of experience. There is the relatively permanent will, the invariable associations; and there is, on the other hand, the shifting of contents and values, the new experiences, the unforeseen obstacles, the pleasant surprises. Why not take experience as it is? But this is not human nature. Our tendency is to emphasize some aspect of the whole and neglect the rest. For instance, you must admit that there is constancy in experience. If that is the case, one argues, there must be absolute constancy. In and through the states, there must be an eternal self, a trancendental substance, which remains identical in all the states. And the changes themselves are merely accidents of this eternal substance or character. And, on the other hand, there is change. Our mental facts come and go. Very well, then there must be absolute change. There can be no constancy if there is change. Each gross moment is really divisible into infinitesimal changes. And everything must necessarily flow through these infinitesimal transitions. There is only appearance of qualities or contents, but really there is this absolute flux. Does the attention vary? Then the contents attended to must vary also. Do our ideas vary? Then our will attitudes must vary too. Such is the reasoning the human mind from age to age has employed; and new editions are appearing all the while. But what we must not forget is that the conception of an eternal self, on the one hand, or of the calculus of flux, on the other, are merely tools with which we work. Their authority in the end can never rise above the facts from which we have derived them. The contents and tendencies may overlap our ideal divisions. All we can say is that in the history of the self there is change and growth and novelty, but there is also some constancy. Else we would not even be talking about flux. There would be no memory or expectancy. We must learn to recognize constancy in so far as there is constancy and flux in so far as there is flux.

Just why there should be such a mystery about the continuous occurrence of a relatively stable context, taking account of a series of successive feelings or perceptions, it is hard to see. But such has been the feeling of others beside Mill: "Accepting the paradox that something which *ex hypothesi* is but a series of feelings, can be aware of itself as a series. . . . I think by far the wisest thing we can do is to accept the inexplicable fact, without any theory how it takes place; and when we are obliged to speak of it in terms which assume a theory to use them with a reservation as to their meaning." [1] Yes, perhaps. But would it not be wiser still not to invent such an absurd paradox? The series of feelings does not as a series know itself, but is known as such by a context of interest which is for the purpose stable. And, again, while our ability to control the series of feelings, so as to keep them in the focus of attention may be circumscribed by a few seconds, it is not necessary to suppose that the interest in controlling them is so limited. The context of interest may be lifelong. To measure the real permanency of the self by the flicker of attention, whether we have recourse to the infinitesimal calculus or finite fractions of seconds is equally mistaken. The real specious present is just as long as the associative interest, determining the series of events, whether we attend to such interest or not. The time-span of the self and the time-span of attention should not be confused. As a matter of fact while attention flickers we can bring back again and again the contents and will attitudes.

The past is not made by the consciousness of it any more than the present. You might just as well say that the geological strata originate with the consciousness of them. The past has its own context and its own content as much as the present. The past comes to have significance for the present moment, when it is attended to; but its own meaning does not originate then. If it did, there would be no possibility of knowing the past, because there would be no past to know. The context of the past must somehow persist and be acknowledged by the present, if we are to have a past. Else

[1] J. S. Mill, *Examinations of Sir Wm. Hamilton*, 4th ed., 247 ff.

there could be no memory. Take the simplest case of recognition, ideal or perceptual. Part of the past content must figure as a content in the present context. This content in definite recognition reinstates its own past setting, be it the sensory context of the perceptual object or the ideational setting, which is acknowledged by the present context, including the dating. There may, of course, be all degrees of reinstatement, and so of vagueness of recognition, but in any recognition a past content must figure as the identical content in the present context together with its own tendencies. In this unraveling of the past context, there is also the more or less vague intuition of pastness, due to the growth series of which the contents are a part. Sometimes an identical content plus this feeling of pastness is all that figures in the present context, and we have the confusion of feeling that we have been in the same situation before, when we know we cannot have been there.

In purposive realization, we have a similar illustration of constancy with reference to the future. If ideas could not persist, but were new every moment and in each infinitesimal fraction of a moment, there could be no such thing as the realization of an end. Experience would be one immediate slough without direction. The facts of experience, however, show that we can keep a constant nuclear aim, however much the context of our meaning may grow in extent and definiteness in the process. There remains an identical content or constellation of content through it all. And so ideal realization is possible.

It is true that we depend very much upon symbols in retaining the past and in fixing the present and the future. Knowing our own meaning, past and present, as knowing those of others, is largely an interpretation of language. But language after all is only the symbol of the contents of experience. Language could not convey the same meaning, unless we owned the meaning. We can recall blue sky when we perceive the words, because we have the actual meaning blue sky, however fragmentary its concrete content. And failing this, as in an unknown tongue, we would simply have

words staring us in the face, conveying nothing. Language, moreover, is discrete and stereotyped and fails to give an equivalent for the quivering transitions that persist indefinitely within the systematic meaning. It is not fair to substitute the tool, however important, for the living reality.

But, we shall be told, the real persistence is not of the contents themselves but of brain-processes. We have already pointed out the absurdity of supposing that our mental contents are converted into atoms and molecules, when we are not attending to them or still more that they should disappear into nothingness to be magically recreated. All that the brain cells can do is what the phonograph or the camera or the written page does for our senses—furnish a record of experience. But just as the mind must furnish the real content for the written page and the other sense records, so it must furnish the content for the brain record. The brain record no more makes the content than the words on the page. And if the brain record *means* a constant record it must be because the content is recognized as the same. As a matter of fact, owing to the uncertainty of the brain record, we substitute largely the ink record which is both more reliable and socially more available. Whenever, then, we have the meaning of identity and not bare physical identity, there must be the identical content. That is the real currency of which the records are the symbols. That we are immediately aware of the brain record and only through the senses conscious of the ink record does not alter a particle the significance of the records. When we read the ink records, we do not read the retinal fibers or light rays; we interpret them as symbols of content, just as much as though they were written on our brain by the law of habit.

If this statement of the facts is true, then if the contents fade or become dissociated, we in the same degree fail to recognize identity in ourselves or others. This is actually true. The mental energies have their own laws of spreading and becoming ineffective as shown by the researches of Ebbinghaus. Sometimes our associative contexts become dissociated; and, when they do, no transcendental ego breaches

the gap. Memory and recognition operate only when there is constancy within the *referent* context. The other contexts, the *referata*, can suggest neither sameness nor novelty, unless there is such constancy in the subject. How far the inefficiency is due to records can be ascertained by the fact that when an objective record is present, visual or oral, the contents are reinstated even when the brain record is ineffective. This fails when there is real dissociation of the context of meaning.

We must, finally, remember that the constancy of the individual meaning is determined not merely by the individual himself, but by his social context of relations, which reacts upon his own consciousness and with reference to which constancy and flux alike become practically significant. This social context of judgment with its records must determine how far the individual feeling of identity can be trusted. The individual meaning sometimes judges itself to be constant, when the social verdict is otherwise. And identity has significance primarily as a social category.

V

The unity of the self is a question distinct from the subject-object relation. Whether the self functions as whole or part, there is, whenever it is awake, the focal and marginal field, the active context and the selected content. This is true even where there is complete dissociation of associative systems. Leone II., when awake, has just as much the I-me character as Leone I., however distinct they may be as regards associations, temperament and character. To what extent, then, the experiences of one organism hang together must be treated as a problem by itself.

The self has as much or as little unity as we must recognize. We must take unity precisely as we are accustomed to take difference—at its face value. And here again no *a priori* assumptions will either increase or diminish the unity which we find in the actual processes. As a matter of fact a transcendental ego or soul, or any other additional entity, spiritual or material, simply becomes one fact more to be related to

the rest; and if we dogmatically deny unity to the facts themselves and treat them as purely unique and disparate, no external linkage will serve as cement to bind the hypothetical differences together, not even in an infinite series. The processes cannot be unified by being "in something" external to themselves, material or spiritual. They must possess their own linkage.

The dogma that each state of consciousness is unique and indecomposable is as indefensible as that of a transcendental knower relating disparate facts. There could be no judgments in an experience where the facts are so intimately blended that the experience could only be taken as a whole. All our thinking depends upon our ability to analyze out identical qualities or relations and to pass from one context to another on the basis of such abstraction, dealing with the contexts only so far as the identical predicate pertains. As a matter of fact actual experience shows that we can take objects and qualities now in one context, now in another, without altering the object by thus subjectively taking it. The content yellow when transferred from the marginal to the focal field of *attention* is not altered in quality, whatever may be the difference of moving it from the marginal to the focal field of *vision*. We can know the past, we have seen, and we can prepare for the future, because we can take the contents and their contexts over again without altering their own meaning or reality in so taking them, however much their significance for the cognitive context may be altered. The dogma of the uniqueness of each state of consciousness as such contradicts all our procedure and would make not only the science of psychology but any science impossible, for all science presupposes the possibility of taking facts over again in experience.

There are doubtless unities in experience which fulfil a unique purpose and which as individual unities cannot be exchanged for other unities. But why, therefore, give our entire experience this character? And even when a whole has uniqueness, it does not follow that the elements cannot be analyzed and taken over again indefinitely. Unique as

the Angelus is as a painting, the elements of color perspective and form can be analyzed out and can figure in any number of contexts. The composition is not unique in its elements, but in the *will*, which they express through their correlation and which furnishes a specific satisfaction to the appreciating subject.

Absolute uniqueness, like the absolute ego, is a dogma unsupported by facts. Why make knowledge impossible by assumptions? If experience came merely by unique throbs, prediction and knowledge would be impossible. If it consisted of wholly diverse contents, knowledge would likewise be impossible. But to some extent we can have prediction, we can pass from fact to fact by means of identities in the stream of processes. Let us take as distinct what experience makes distinct and what experience joins together let no man's assumptions put asunder.

If we insist on abstracting from the processes of experience altogether—from its active context and its selected datum—in order to discover something which is not content at all but which is super-added to the activity of the self, we get not a transcendental ego or any other common entity outside of the process, we get the fact of consciousness in the abstract which accompanies all our apperception. This is not a spectator. The spectator of experience must be the apperceptive context. It is merely the condition of awareness at all. It is quite colorless. It is responsible for neither diversity nor unity. It holds together as little as it separates. It simply makes the facts apparent. It is not necessary to give consciousness an individual existence for each knower as is done with Purusha in the Sankyah system. The individuality belongs to the processes themselves, in their phylogenetic and ontogenetic history. Consciousness taken as an abstraction is homogeneous.

Sometimes we must recognize the self as partial. It may be a case of impulsive control where the total context of tendency fails to express itself. We then speak of ourselves in retrospect as not being ourselves. It may be a dissociation of memory, which makes us fail to connect with the past

and so carry out our obligations and satisfy expectations. It may be organic disorder which makes one part of the content, previously linked with the self-content seem foreign altogether, with altogether strange properties. It may be a case of more profound dissociations, where disrempted systems of association, each with its own characteristic tendencies simultaneously or alternately, struggle for the mastery. In all of these cases the thus dissociated contents may come to figure again, after restored equilibrium, in a total context with its characteristic consciousness of identity. But, in any case, we must accept the actual association or dissociation as it is.

Even when the facts hang together, they do not hang together in the same way. There are different grades of unity. This has not always been recognized. Because some facts hang together systematically as parts of a purpose, it has been argued that the self is fundamentally a thinking or rational self and that all mental activity is implicitly or explicitly of the judgment type. The realization of an ideal self, then, becomes a case, not of empirically selecting and composing a unity in obedience to a purpose, but of becoming conscious of an eternal self already constituted and implicit in our simplest mental acts.

Again, because some facts apparently hang together only externally, in space and time, in our attention moment with no seeming internal bond, but simply interpenetrate or stay side by side in the field of interest, it has been argued that habit or contiguity is the only real linkage of our mental facts, with similarity given perhaps a more or less vague secondary place. Or, perhaps, the opposite: since similarity of content is sometimes found to be the seeming linkage, contiguity is made a case of similarity, if the two are not simply held out as irreducible laws. This external linkage, moreover, has generally been credited, not to the side of mental processes, but to the account of brain processes. Our ignorance of brain dynamics and the paradox of cementing feelings and ideas by means of atoms and molecules and their habits has not discomfited physiological psychology.

Now here again we must take connections at their face value. In part, evidently, the facts of mind hang together as members of a system. They cohere and are controlled by an identical purpose—utilitarian, logical, esthetic, ethical or religious. It may be the idea of wealth; it may be the pursuit and creation of beauty; it may be the love for truth; it may be the passion for righteousness; it may be the imitation of Christ. The ideal self is no doubt the self unified within a comprehensive purpose, where all claims are adjusted, all facts seen in systematic relation. But for us finites that is largely an aim and only in part fact.

The linkage sometimes is the consciousness of a common quality or relation by means of which the otherwise seemingly heterogeneous facts or contexts hang together. Thus consciousness of a common quality may bring together processes which have never been experienced together and so is quite a distinct and elementary fact. In every case of recognition, however rudimentary, this consciousness of partial identity of traits of one individual or context with another is present, This, in the case of the judgment of analogy—the building of expectancies upon identical traits—forms the transition to the reading of facts by internal connections rather than adventitious. That one individual or setting is merely like another is an adventitious relation. It is when the identity links them in a system of prediction, that we have science.

The loosest or most adventitious bond is that of habit, the contiguous interest in facts merely happening together in space or time. The overlapping part of one context will tend to reinstate its other context or one of its other contexts in accordance with the strength of the habit so established and the dominant set at the time. The set of active tendency acts like a switch system, making some habits effective and others ineffective for the specific purpose.

Whether the linkage be through a systematic purpose or through similars or through habit, in any case the linkage is due to the consciousness of identity in the variety of facts and contexts. There must be at least the bond of unitary interest, whatever may be the own linkage of the facts them-

selves—the identity of the context which takes account of the facts, fleeting and heterogeneous as these facts may be. While a great deal of our mental life is held together by such contiguous interest, we must not forget that it is precisely in the mental world that we can follow the transitions from fact to fact and have the immediate consciousness of their string of identity, *i. e.*, wherever we have purposive unity. Outside of what appears to be mind we must be satisfied to piece out the transitions conceptually and inferentially. We cannot follow immediately the transitions of the facts. It is a mistake to ignore these internal transitions of the mind and to strive, as modern psychology has done, to reduce the relations of mind to the adventitious and external kind. Purposive unity must be recognized together with passive association by similars and by contiguity as an actual type, as well as an ultimate ideal, of mental connection.

VI

What becomes of activity and freedom, if we once admit that the stream of tendency is the agent? No theory can unmake the facts and such activity or freedom as there is still remains. It is hard to see of what use a transcendental knower or any spiritual something could be in accounting for activity or any other function of the self, as it must necessarily be the same whether the self is active or passive. Activity, as contrasted with passive and non-voluntary states, would still have to be stated in terms of the concrete processes, though somehow it is hard to rid ourselves of the feeling that the idea of an extra entity, added to the facts, affords an additional guaranty of identity, unity and freedom.

Activity has sometimes been identified with the unpredictable and novel. The world of psychic reality, it is rightly pointed out, is not exhausted in our concepts. It is not all included in our present logical context of meaning. One characteristic fact about the stream of consciousness is its ever novel situations and novel attitudes. You cannot but partially predict the future. But however true this may be, as stating our finite experience, and however much we may

recognize the truly temporal aspect of life, it does not seem possible to define activity in such terms. For change and novelty exist apart from our activity. They confront us whether we are active or passive, awake or asleep. They may come to thwart our purposes as well as come as the fruits of our efforts. I do not see, therefore, how we can define activity or freedom in terms of novelty.

Moreover, activity and freedom must mean the realization of an aim. And we cannot aim at the unpredictable. We can only wait for it and let it happen as it may. Activity on the contrary means the control of events—ideas, feelings, perceptions, impulses—by an idea which remains constant. It is just this conscious leading that we mean by the self, in our awake moments. That the novel and unforeseen happen is incidental to the activity and may be forced upon us quite independently of our being active; though a general readiness even here is the thing, in turning the novel to our advantage when it comes. Activity means that the consequences follow from our intention, not contrary to our intention.

Activity, therefore, is the very opposite of chance, which means the unpredictable and uncontrollable. Fluent our world must be to some extent to have expectancy, to look to the future; and with the fluency there may be novelty. Perhaps there always is. But freedom relates to making real the ideal content, to regulate the flow in accordance with the dominating purpose. Only as the idea can recognize the results as its own fulfilment, however much more definite and concrete, can we have freedom. Whether the flow in such a case is determined altogether by 'considerations,' or whether it is to some extent independently variable, does not concern the question of freedom, since we are only free in so far as the flow *is* controlled by the purpose, *is* organized into some ideal scheme of life. The novelty, as we finites can take account of it, is necessarily an afterthought, a gift of the process, and by hypothesis it is not anything we can theorize about. It is a character of the concrete flow, as contrasted with the expectant and guiding idea. Whether it is absolute novelty

or due to the peculiar limitations of our experience it is difficult to know. Some of it is evidently due to the limitations of our finite consciousness, where we often discover that what is novel to us as individuals or even as an age is already part of the content of historic humanity. The novelty of the child, to whom all is novel, is merely its taking over of the content of the race, barring its own specific organization. But while novelty abounds in the infant's life, there is no freedom. Conduct is free only when it is a consequence of a systematic purpose. We don't call a man free in so far as he must say of the outcome of his conduct: "That is not what I meant" or "I had not thought of that."

VII

Nor does this theory destroy the value and worth of conduct in so far as it possesses those characters. Any abstract entity added to the processes would as little account for the presence as the absence of value in any specific process. Value is a function of activity. Objects have value when they satisfy some tendency of the self, in its various stages of complexity and equilibrium. As every satisfaction of the will is a value, we must distinguish between value and worth or subjective value and objective value. Whether an activity has worth or not does not depend upon its individual satisfaction, but upon its agreement with a standard which the will must acknowledge. It may be the standard of social agreement; and this at any rate is enforced upon the individual will, whether accepted by it or not. But as this too is variable, our finite activity, in order to have worth, must refer to a standard which the social will, too, must accept in its racial development. The meaning of this we can only catch gradually, and every such advance in meaning must come from individual insight.

As the identity of activity with an objective standard is worth, so, if there is an ultimate and eternal standard, agreement with this standard means immortality. As society conserves the unities which harmonize with its standards, and makes the poem, institution or individual that expresses its

will immortal, in so far as in it lies, so, if there is an ultimate standard and a will to enforce it, this will must intend the immortality of that which realizes the standard, be the unity personal or impersonal. The worthless unities could not, in such a world, survive as individual unities, they could only survive as contents or tendencies to be used as raw material for more comprehensive unities, as the button moulder in Ibsen's Peer Gynt melts up the sham individuals in his ladle.

DISCUSSION

PROFESSOR TITCHENER'S THEORY OF MEMORY AND IMAGINATION [1]

The theory of memory and imagination of which this paper proposes a partial discussion cannot be understood without a clear comprehension of the terms in which the author carries on his exposition. It is to these terms, therefore, that we shall first give our attention.

The division of the *Text-book of Psychology* that we are concerned with in this review deals with the recognitive, the memory, and the imaginative consciousnesses. Titchener's exposition of each of these processes falls under the three following rubrics: the "focal process," the "essential factor," and the "typical forms." The term "focal process" indicates the character of the process that is central in a given case. In memory and imagination this is ideational, while in recognition it is perceptual. The term "essential factor" means "what is characteristic of" the consciousness under consideration. By "typical forms" or "types of consciousness" is meant the kinds, or classes, or species of the consciousness being considered.

A partial presentation of Titchener's view of these three forms of consciousness will be the following:

The Consciousness	Focal Process	Essential Factor	Typical Forms
Recognitive consciousness	Perception	Feeling of familiarity	Definite
Lack of the recognitive consciousness	Perception	Feeling of strangeness	Indefinite
Memory consciousness	Image	Feeling of familiarity	Remembrance Recollection
Imaginative consciousness	Image	Feeling of strangeness	Reproductive Constructive

If, now, we consider what Titchener has to say about each of the "typical forms" of the memory and imaginative consciousness, we are led to another set of terms. Two passages are immediately important. Remembrance and recollection, the "typical forms" of the memory consciousness, the author says, are "discursive; that is, are characterized by wandering of attention, shift of imagery,

[1] *A Text-book of Psychology*, pp. 396–427.

variable play of association." Reproductive and constructive imagination, the "typical forms" of the imaginative consciousness, are "integrative rather than discursive; the sphere of attention is limited, the play of association regulated." An interesting, and for the view under consideration an important, question is suggested by this way of stating this difference. Are we to understand that "discursive" and "integrative" are additional "essential factors," or are they something distinct? By an "essential factor," as we saw, was meant what is "characteristic of" a process, and in the passages just quoted, "discursive" and "integrative" are interpreted in terms of what these processes are "characterized by." Are these two phrases, "characteristic of" and "characterized by," synonymous? If so, the "feeling of familiarity" and the "feeling of strangeness" as the essential factors of the memory and imagination consciousness, respectively, need to be supplemented by the other pair of terms to make the former account complete. If this is not the meaning, have we here a shifting of the ground, that is, a change in the point of view from which description is taking place? The meaning assigned to the terms "discursive" and "integrative" suggests that this is the case. For by these terms we are made acquainted not with what the memory and imaginative consciousness are, but with what they become. They are interpreted in terms of attention and the play of association. And, in harmony with this interpretation, we are told in the immediate context that each form "shades off" into other related though distinct processes, and later that these processes, which I shall refer to as "genetic forms," constitute what the author calls a "psychological circle." Unless "discursive" and "integrative" mean something genetic, it is difficult, if not impossible, to interpret the context in which the distinction occurs. If this is the author's meaning, and if we may use the sign → to indicate the phrase "shades off," we may represent the doctrine in the following schema:

The Consciousness	Genetic Characteristic	Typical Forms	Genetic Forms
Memory consciousness	Discursive	Remembrance → Day dreaming → Imagination Recollection → Inquiry → Thought	
Imaginative consciousness	Integrative	Reproductive Constructive → Thought	

So much for the essential features of the doctrine. In what follows we shall be concerned with the meaning of what Titchener says. This will necessitate a constant reference to the many details

that necessarily have been omitted from the schemas above, and will go some way toward mitigating whatever injustice has been done the view by its schematic representation. But in the main we shall confine our remarks to the problems set by the two tabular views given above. And the first question is, what does Titchener mean by memory and imagination.

To start with the memory consciousness, we have here, according to the author, a process that is identical with the recognitive consciousness with the exception that the "focal process" is an idea and not a perception (p. 413). What, then, is the recognitive consciousness? It is a form of consciousness that is dependent upon the "secondary effects of (sensory) stimulation." These secondary effects "give us the key to the psychology of recognition" (p. 407). The secondary effects referred to are (1) organic reaction, and (2) feeling. Sensory stimuli, Titchener holds, set up both of these in addition to arousing sensations, and it is upon this fact that he bases the distinction between the "typical forms" of the recognitive consciousness (p. 409). If now we apply these statements, *mutatis mutandis*, to the memory consciousness, we may suppose that it also is dependent upon organic reactions and feelings, and that these are mediated by an idea. This, at any rate, is what it would be if, as the author says, "the sole difference is the presence of an idea in the one case and a perception in the other" (p. 413). But to take the passage immediately following the last quoted, "An idea is a memory if it is accompanied by the feeling of familiarity; and an idea is specifically remembered if it is placed and dated by the organic reaction and by associated ideas." Now, apart from the possibility, which seems to be granted, that we may have a memory which does not involve organic factors at all, we may ask why the statement has been given the hypothetical form. The reason is, according to Titchener, that "no image or idea is intrinsically a memory-image or memory-idea" (p. 413). The meaning, accordingly, is that when an image or idea is accompanied by a feeling of familiarity it is a memory, and when an image or idea is accompanied by organic reactions and associated ideas which give date and place to it, it is a specifically remembered image or idea.

One or two questions suggest themselves in this connection. The most concrete, if not the most obvious, inquiry relates to the method by which the organic and feeling factors that accompany the memory consciousness get aroused. If we subtract the organic and feeling elements from a memory consciousness there is left, presumably, an image which is not intrinsically a memory-image. As the author

says, "an image is made into a memory-image by the feeling of familiarity" (p. 419). If the image is not intrinsically a memory-image, how, psychologically, are we to regard this addition that is essential to the existence as a memory consciousness? In the case of the recognitive consciousness the matter is clear. For there, whatever arouses the sensation arouses at the same time the other elements that give the recognitive character to the experience in question. It would appear that it is different with the memory consciousness. Or may we say that whatever arouses the image arouses at the same time the other elements that impart to the experience its character as memory? If not, does the *Text-book* provide a different answer? It should be observed, to avoid misunderstanding, that our question is not met by what the author has to say on p. 414 f. where he is speaking of the pattern of consciousness in remembrance and recollection. The difficulty that the question is intended to call attention to arises, we think, from the author's identification of the memory consciousness with the recognitive consciousness with a single, which is said to be a sole, difference, and from his inability to meet the expectations aroused by this statement in the further study of the memory consciousness. In the detailed study, the memory consciousness seems to get separated by a greater distance from recognition than is compatible with the earlier account of their relations, and several differences, instead of the original one, seem to develop. What one notices here is a tendency to unite memory with a process—recognition—that has a sensory basis, and, then, to react gradually from, or to refine the position until one is left wondering whether these processes have anything to do with one another. This backward and forward movement, we may note by the way, is quite characteristic of the author's treatment of most of the topics with which we are concerned in this paper.

Another question, more general in its nature, and connected with the foregoing, relates to the standing of the memory consciousness itself. As we come away from the reading of Titchener's exposition we wonder whether there is any such thing as memory which is not either a remembrance or a recollection. This view is, of course, what we should expect from a psychological study. For psychology, there is no such thing as memory, there are only memories. And these memories are, in the terms of the *Text-book*, either remembrances or recollections. This view, however, does not accord with the exposition, for, as we have seen, memory differs from the specific memories by the absence of the organic factors that characterize

remembrance and recollection. Memory, moreover, is set off from its "typical forms," in a positive way, by the presence of the feeling of familiarity. Both positively and negatively, therefore, memory is distinguished as a separate process from the kinds of memories that one may have. We are not saying that this may not be; we are only wondering whether this is Titchener's view, or whether we are to take memory as a class term which is logically distinct from, but psychologically is identical with the items that are classified under it. We may remark by the way that the same line of comment applies to the author's use of the term imagination, and to the exposition of this topic we may now turn.

The main views of the author concerning imagination need not detain us long because the treatment here follows closely the lines laid down in the section on the memory consciousness, and because, as the author says, "we know very little indeed about the imaginative consciousness." Reference to the first table given above will show that an image or idea is a case of imagination when it is accompanied by the "feeling of strangeness," and we are told in other places (cf. pp. 417, 423) that with respect to the organic factors involved imagination is distinguished from memory by the fact that these are in this case empathic rather than kinæsthetic. Further, the author holds, although he does not state it explicitly, that in this case also no image or idea is intrinsically an image or idea of imagination, and he definitely affirms that "an image is, psychologically, made into an image of imagination by the feeling of strangeness" (p. 419). In this process, as also in the memory consciousness, it would seem that if we were to strip off the feeling and organic features we should recover a residuum which in both instances is the image, and if this is the fact, it is doubtless necessary to examine the doctrine of the image as a means of understanding the theory of memory and imagination that is now before us.

We have spoken in the foregoing as if the term image were univocal and in doing so we have merely followed the author in the passages referred to. In this paragraph we shall present, as far as possible in the words of the *Text-book*, the meaning to be assigned to this term. The fact that the important passages are widely separated has affected, if not the doctrine, the form in which the doctrine is presented. These circumstances under which we are compelled to approach this topic make it barely possible that we may at times have missed the essential meaning. However this may be, the important passages are on pp. 48, 197–198, 420–421. The first passage

occurs in connection with the question of the nature and number of the elementary mental processes. As this is a somewhat fundamental and important reference, it will be well to consider in what sense any mental process can lay claim to be for psychology an element of mental life. There is nothing obscure in this, and the author, after stating that by elements he means "the simplest materials out of which we are to build up our entire psychology" (p. 46), goes on to say that such materials, which are "actual items of mental experience" (p. 50), must be "strictly elementary, they must remain unchanged, however persistent our attempt at analysis and however refined our method of investigation" (p. 46). Indeed, it is the business of psychology "to describe and explain these elementary processes, and to show that, when grouped and arranged in certain uniform ways, they give rise to the different complex processes that constitute human consciousness" (p. 48). Such an elementary process the image is affirmed, by Titchener, to be. He writes: "Images are, in just the same way (as sensations), the characteristic elements of ideas, of the mental pictures that memory furnishes of past and imagination of future experience." Now if we ask what the nature of such an image is, we are told that the "image differs from the corresponding sensation in three respects, its qualities are relatively pale, faded, washed out, misty, and its intensity and duration are markedly less" (p. 198). These differences are, however, "differences of degree, and not of kind"; and if we ask the reason for this the answer is, as the passage suggests, that there is a correspondence between sensation and image and, as is stated elsewhere, the image arises later than its corresponding sensation, that is, is in some way dependent for its existence on the prior existence of the sensation. Here there is a tendency to assimilate the image with sensation, a tendency that has been current since the time of Hume. In the language of Külpe, what Titchener asserts is that the image is a centrally excited sensation, the sensation, of course, depending upon peripheral processes. As a further statement of opinion, the following passage is important: "But the writer is not sure that the image does not, as a rule, evince a sort of textual difference from sensation, that it is not more filmy, more transparent, more vaporous" (p. 199). This passage, we confess, is difficult to interpret. Moreover, its context does not clearly indicate its probable meaning. It occurs in a discussion of the question why, since the image differs in degree and not in kind from sensation, we do not more often confuse the image with sensation, if that is the fact, and the answer

is that this must be accounted for, in large measure, by "the differences of conscious context or setting in which the two processes occur." Then follows, by way of illustration, the statement that images are "less localized than sensations; they change and shift more rapidly and in a meaningless way; they move with the movements of the eyes. "But," the text continues, " the writer is not sure that the image does not, as a rule, evince a sort of textual difference from sensation, etc." What does this "but" mean? Does it mean that after all we are not dependent for our distinction between sensation and image on the criteria by which they are marked off as differing in degree and not in kind, but that we may find our criterion in the image character itself? A passage seeming to support this interpretation occurs on p. 365. If, then, it means this are we to suppose that this is true only within the "conscious context or setting," or does it apply also to the image under the standard conditions that determine its nature as an elementary process? If the former, it is not a statement that applies to the elementary imaginal process, but, as turns out later, to the images of memory and imagination; if the latter, an intrinsic difference has been pointed out, but we do not know whether this takes the place of the extrinsic distinction that occurs on the preceding page. Because, then, of the uncertainty of the conditions under which the statement is held to be true, we are in doubt whether the image has the sensuous qualities that assimilate it to sensation, or whether it has the textual difference that makes it intrinsically distinct from sensation. And if the author refuses the alternative and accepts both statements, it still remains to be shown how each of them can be true of an elementary process.

A great deal in Titchener's view of the image doubtless reminds the student of the history of psychology of the very similar way in which Hume has dealt with the image, and he will be inclined to recall the historic statement of the British empiricist that the image "strikes the mind" in a way different from sensation. He will also doubtless recall the passage in Stout's *Manual* (p. 397) where it is suggested, correctly probably, that by this phrase Hume intended to draw a qualitative, and not merely a quantitative, distinction. Whether this is consistent with the thoroughgoing sensationalism of the *Treatise* need not be discussed; but one is a little surprised and perplexed to find that Titchener does not avoid but aggravates the difficulty of the earlier writer, and we have to leave the former's treatment without any clear notion as to whether, as an elementary

process, there is any such thing as an image. His first passage tells us that there is; his second presents the image in a character which, in his own meaning of the term, is not "strictly elementary"; and his third passage raises a doubt as to both the other two. This illustrates, in another connection, the vascillation in statement if not in view that has been already referred to. For as we have seen above after assimilating the given process (the image) to sensation, something occurs in the exposition to remove it from this connection and to give it a quasi-independent standing. Until we know whether these later passages point out an intrinsic or extrinsic difference, we do not know how seriously we are expected to take the image as an elementary mental process.

The last of the three main passages referred to above occurs in the section "The memory-image and the image of imagination" (§118). We include the passage here because in the section the author states that the paragraph has to do with the elementary image. He writes: "It is clear, from what we have learned of the imaginal complexes in memory and imagination, that the elementary process, the image of §61, has two distinct forms . . . the image that may be confused with sensation" and "the image that is of filmier texture than sensation" (p. 420). This statement appears only to repeat a distinction already drawn on p. 199, and this we have considered. There is, however, this difference that whereas the author, in the earlier place, was stating the relations that the image has to sensation, in the latter he arrives at the statement through a consideration of the characteristics of the image of memory and imagination. All the difficulties, therefore, of the first passage press upon us here, unless the term "forms" is to be taken as an admission of the elementary image in a double character. If this is what is meant, it is not clear how this will comport with what Titchener says of the conditions that any process must fulfil to be an elementary process of mental life. When, however, in the immediate context we are told that these images appear respectively in the images of imagination and memory, we wonder whether this is intended to do away with the distinction between the elementary and developed processes of the imaginal type. That is to say, if images of imagination and memory are forms in which the elementary imaginal process exists, how can it be true, as we saw above, that this latter is "made into" the former by the supervention of characteristic emotive processes? The elementary image, thus, comes before us once again in an equivocal character, and we may repeat that until the status of the

elementary image has been fixed, it were fruitless to take *au sérieux* the author's theory of memory and imagination.

If this paper were not already long, it would be possible to show that the main difficulties of the author's exposition are due to the conflict between two points of view which are not always distinguished or under control. We cannot avoid the impression that while structural motives are uppermost, genetic considerations are operating in many places and in important ways. To test this supposition, it would be necessary to examine in detail the various characteristics that have been assigned in the text to the image of memory and imagination (cf. pp. 417, 421, 424). We must remain satisfied with a partial reference to a single statement, and that a quite general one. The image of memory and imagination, we have seen, has been described respectively as "discursive" and "integrative," and the author adds that "the consciousness in which the memory-idea (p. 413) and the idea of imagination (p. 422) is set may share the pattern either of primary or secondary attention," and attention for him is capable not only of a physiological but a genetic statement (pp. 271, 272). By genetic in this connection is meant that racial history has in important respects determined the characteristics under consideration. The more careful statement on p. 273 recognizes the influence of education in addition to that of heredity in determining the specific reactions. Now under education we must include not only the modifications of racial inheritance by acquired reactions, but also the results of such modifications as are seen in those cases where significance for the individual comes to attach to particular concrete environments. To take Titchener's example, the idea of working for an examination is the important focus of a complex situation—in which the tendency to follow a fire engine appears as a disturbing factor—*for the student.* It is not so for the man who, although he were working for an examination, happened at the same time to belong to the fire department. In this case there would be, through interest or what not, a change in the total situation which, to the onlooker, would be identical in the two cases. We refer to these possibilities because they emphasise the importance of taking into account elements and factors of problems that are given only a onesided statement in a purely physiological or experimental psychology. It is regard for these factors that gives to genetic psychology its distinctive task. For genetic psychology can take neither the environment apart from the subject, nor the subject apart from the environment, but considers environmental and sub-

jective conditions as continuous and interrelated factors in each mental complex. This statement is illustrated in Titchener's treatment of the "discursive" and "integrative" characteristics of the imaginal consciousness, for as we saw, these characteristics are due to the movements of the attentive, imaginal, and associative factors present in this type of experience. Memory and imagination are mental complexes whose character is to be read in the rôle played by the above-mentioned elements under the conditions that are present in the specific case. But when the patterns of attention are carried over into the memory and imaginative consciousness, these latter are definitely set before us as problems in genetic psychology. And if this is true, we have a point of view for interpreting those features of the doctrine that involve the idea of development, those, for example, that have been summarized in the second table above. In particular, the assertion that the image is "made into" an image of memory and imagination, and that the "typical forms" of each of these "shade off" into the other forms of the conscious experience, are, if they are anything, genetic statements; but they do not seem to have been brought, in the exposition, into explicit and clear relation with the point from which the discussion starts out and which is concerned to maintain the primary elementary character of the image. We are, consequently, left with this confusion: either the image fulfils the requirements of an elementary process and then it cannot become anything else; or, the image undergoes characteristic developments and then it is not elementary in the author's meaning of the term.

We may refer to one other point in conclusion. The author says that there is a continuity between the "typical forms" and the "genetic forms" of the memory and imaginative consciousness in the sense that when the one "shades off" into the other a "psychological circle" is formed (pp. 414, 422). Does this mean that if we start at any point in the imaginal consciousness, say with remembrance, it is possible by a series of progressive changes to run our line through all the other forms without a break? If this is what is meant, does it not imply a distinctively genetic view of the imaginal consciousness? But if this is the meaning, we have to confess that we have failed in our attempt to do this, and it would be interesting to know whether Titchener has been more successful. If he has, we should have to acknowledge his genetic view, but should have to maintain that his conception of memory and imagination is, in important respects, defective, and that parts of his exposition are irrelevant. For we again insist that if the account is genetic it is

difficult to see what part the elementary image has to play in the process; and if it is genetic in the sense of being circular, the account is defective in not recognizing the divergence between a memory and an imaginative consciousness.

We close the discussion with three remarks: (1) Where structural motives are in control, the author shows minute knowledge and clear appreciation of the problems in hand, and (2) where genetic considerations are uppermost there is much with which the writer is in cordial sympathy. But (3) when we try to find out the relations between the foregoing points of view, the *Text-book* fails to give the proper guidance, and this reacts unfavorably upon the statement of the author's views.

ARTHUR ERNEST DAVIES

OHIO STATE UNIVERSITY

MEMORY AND IMAGINATION: A RESTATEMENT

Professor Davies has allowed me to read the manuscript of his critique; and the Editor has invited me to reply in the present number of the REVIEW. These are courtesies which I gratefully acknowledge. Psychology is still in so unsettled a state, that everyone who tries his hand at systematization must welcome criticism. When, however, the criticism deals less with scientific method and result than with interpretation of a text; and when the critic writes from a point of view which is foreign to the author criticized; then it seems but just that the reader should have the two statements, objection and rejoinder, laid before him at the same time. I am therefore glad to avail myself of the opportunity, which Professor Davies and the Editor have kindly afforded me, to discuss certain passages of my *Text-book*.

I begin with the Image as psychological element. The *Text-book* is intended for use in the class-room; it is meant, that is, to be read in order, chapter by chapter; and its later sections are always to be understood in the light of the earlier. The first section that deals with the imaginal element is §10. Here I point out that psychologists are not yet agreed upon the nature and number of their elements; that there is, however, a fairly definite trend of opinion; that we may therefore proceed, in the *Text-book*, upon a certain assumption; and that, if the future brings change, this will probably be a change by addition and not by subtraction. The assumption is that there are at most three elements; that two of these (sensation and image) may be considered as sub-classes under a general heading, even if they may not be grouped outright in a single class; and that all three (sensation, image, affection) may plausibly be reduced to the same ultimate type. I then characterize images, in a sentence, as the elements of ideas; I say that they are often confused with sensations; and I refer forward to §61.

I have, then, "affirmed the image to be" an elementary process; but I have not, at this stage, affirmed it to be an elementary process distinguishable, in psychological analysis, from sensation; on the contrary, I throw out the suggestion (new to the beginner in psychology) that sensation and image, the characteristic elements of perception and idea, are not seldom confused. When the issue is

expressly raised, in §61, I say—not, as Mr. Davies makes me say, that "the image differs from the corresponding sensation in three respects," but something very different; my statement is: "It is usually said that the image differs, etc." I thereupon proceed, on my own behalf, to argue that the alleged differences, being differences only of degree, cannot be admitted as final; and I show that, in fact, they are not adequate to a differentiation of image from sensation. Why, then, I ask, do we not confuse image and sensation in daily life? I reply, first, that we probably do confuse them far more often than we realize,[1] and secondly that we may in many cases distinguish them, not by intrinsic difference, but by difference of conscious context and setting. So far, therefore, I still favor the identification of sensation and image. But now comes the mention of a possible intrinsic difference. "The writer is not sure that the image does not, as a rule, evince a sort of textural difference from sensation; that it is not more filmy, more transparent, more vaporous. If this is the case, then it is better to consider sensation and image as sub-classes of a particular type of mental element than to include them outright in a single class." "This passage," says Mr. Davies, "is difficult to interpret." Yet it is, I think, susceptible of only one interpretation. I recommend the reader not to identify image outright with sensation; there is some evidence of an intrinsic, attributive difference between the processes,—a difference which I do not name, since naming would be premature; but which, if its existence is verified, will have to be called 'modality' or 'substantiality' or something of the sort. The discussion of the 'relations' of sensation and image (p. 48) thus ends in a *non liquet;* though I indicate—if the personal matter is of any importance—that I am more disposed to separate sensation from image than I was when I wrote the *Outline*. The elementariness of the image, of course, is nowhere called in question.

I come now to §118, which is based upon the results of Perky's experiments. I confess that these experiments gave me a good deal of trouble. I believed that their conclusion was valid; but I foresaw that it would not be generally accepted without confirmation from other quarters; I realized that suspense of judgment was scientifically justified; and I felt that I had therefore no right, in a text-book, to press the conclusion to its logical consequences.[2] What I did was

[1] This point is illustrated further on, in the fine-print paragraph of p. 365, where a back-reference to the present p. 199 is given. There are, of course, no text-references from coarse to fine print; but the Index covers everything.

[2] Professor J. R. Angell has, in fact, taken me to task for admitting the experiments (*Philosophical Review*, XX., 1911, 547). But in such cases a writer must use his own udgment.

to give the outcome of Perky's work, in fine print (pp. 420 f.), and to leave the reader to draw his inferences as he pleased. It is clear, I say, that the experiments give us two elementary 'images,' two kinds of 'characteristic elements of ideas.' The one is indistinguishable from sensation; it is imaginal only in the sense in which popular psychology speaks of images; in strictness, it is not separable from sensation. The other is "of filmier texture than sensation,"—an obvious reference to §61. I try to account, physiologically, for the first kind of 'image'; but there I stop. And I fail to see in the exposition anything like obscurity or contradiction. The coarse print of §61 says: 'Image is often confused with sensation, but still there may be an attributive difference: keep your minds open.' The fine print of §118 says to the instructor: 'Here are the experiments (417 f.), and here is what they tell us of the elementary image (420 f.); they bear out what was said in §61 of a possible intrinsic difference from sensation (the element of the imaginal complex in memory), while they also account for the confusion with sensation (the element of the imaginal complex in imagination).' If the instructor accepts the results, he will make his teaching more positive than §61; if he does not, he will offer his criticisms, and allow §61 to stand as written.

I hope that I have herewith met Mr. Davies's difficulties with regard to the elementary image. I find two, plainly distinguishable elements: sensation and affection. I find another alleged element, the image; this is set off from sensation, in the books, either by reference to a central origin or by difference of attributes. The origin is of no concern to descriptive psychology; and a comparison of attributes leaves us in doubt whether we should operate with sensation alone or with sensation and image as distinct processes. If, now, we accept certain experimental results, and interpret previous experimental work in the light of these results, the situation clears: what has been called the 'elementary image' turns out to be two things, an elementary process indistinguishable from sensation, and an elementary process modally different from sensation. The former 'image' is, for systematic psychology, a sensation; it disappears as image from our discussion; the technical term 'image' is reserved for the second type of process.

Further than this, I cannot "fix the status of the elementary image"; the 'if' is there, and cannot be got rid of. I proceed to the psychology of Memory and Imagination.[1]

[1] Mr. Davies entitles his paper "Professor Titchener's Theory of Memory and Imagination." I am, however, but little occupied with theory; what I attempt is a descriptive psychology of the two topics.

In §117 (which comes before §118, and therefore depends for its doctrine of the elementary image upon §61) I begin with the statement that "no image or idea is intrinsically a memory-image or a memory-idea." Neither does analysis reveal a specific element 'memory-image,' nor is any imaginal complex in its own right a 'memory.' Ideas are 'made into' memories by a context; not genetically, of course,—I do not raise that question,—but descriptively, as a matter of analysis and synthesis. The context itself is in memory what it was in recognition; "the sole difference" between the two consciousnesses, in cross section, is that "the focal process is an idea and not a perception." In recognition (§115) the contextual 'feeling of familiarity' may be regarded as a particular derivative of the secondary effects of sensory stimulation; these effects, in general terms (§60), are associative processes, organic sets and attitudes, and affective processes. I do not attempt to describe their nervous correlate. Mr. Davies now asks: How are the organic set and the affective process aroused in the case of memory? And the question, if I understand it aright, means: Are they aroused directly, as secondary effects of the central stimulation, or only indirectly, by way of association? It is, in other words, a question of nervous correlate. As such, it does not concern me; and I do not remember that I considered it, when I wrote the section. It is, however, an interesting question, because psychologists, when they are dealing with centrally excited processes, usually take the associative mechanism as a matter of course. I think—if an opinion given without much time for reflection is worth anything—that the associative mechanism (in the sense of §§105–111) is adequate to the memory context; and I think there is experimental evidence to show that the organic reaction to perception is, other things equal, at any rate more widespread and more intensive than the reaction to idea. But since central stimulation is still stimulation, and the organism is still a system, I should imagine that secondary effects of some range and of some intensity are inevitable.

I recognize no memory other than that given in remembrances and recollections; I believe, however, that memory, like recognition, may be definite or indefinite (pp. 409, 413). In the same way, I recognize no imagination (§119) other than that given in particular cases of imagination. And as with memory, so with imagination: an imaginal complex is 'made into' an 'image of imagination' by a context, by a peculiar 'feeling of strangeness.' The opening paragraph of §119 is written with direct regard to the opening paragraph of §117. The terms 'discursive' and 'integrative,' which Mr. Davies finds

so disquieting, are meant—I was on the point of writing 'of course' —to give a temporal, not an areal, characterization of the two consciousnesses. It did not occur to me, I admit, to attach the labels ("If we look at the consciousness in longitudinal section," etc.); but I hardly think that, at this stage of the book, the labels are needed.[1]

There remains the "psychological circle" of pp. 414, 422, which Mr. Davies again views through his genetic glasses. My standpoint is still purely descriptive. I want to impress upon the student that the comprehensive terms of descriptive psychology are names of type-consciousnesses only, and that between the type-consciousnesses there are all manner of intermediate forms. Put memory, then, at the north pole of a circle, and imagination at the south pole. Starting, on the one side, from memory as remembrance, we pass through day-dreaming to reproductive imagination; starting, on the other side, from memory as recollection, we pass through search and thought to creative imagination; the former is the side of primary, the latter the side of secondary attention. The diagram serves to give the student his bearings among certain gross terms of current psychology, and also, by the continuity of the circle, reminds him that his everyday consciousnesses are probably not as clear-cut as the consciousnesses of his text-book. I do not think that much is gained by the elaborate figures and formulas which many psychologists use in their description of the 'higher' processes; but this particular diagram is very simple; and, if it is not necessary, I hope at least that it is harmless.

I have now replied to what I take to be the essential points of Mr. Davies's criticism. The criticism is, throughout, of the immanent kind; my exposition of the psychology of Image, Memory and Imagination is considered as a whole, without regard to pedagogical order; and, aside from the reference to Hume and Stout, the experimental and historical sources from which my data are obtained do not come

[1] Here, *e. g.*, are two sentences from p. 423. "In memory, the observer is always within a certain universe of discourse; there are limits, set by the fixity of past occurrence, which he may not transgress, but within this breadth of context he can move at will; consciousness is discursive. In imagination, consciousness proceeds, as a whole, from the fountain-head of disposition; there are no limits of any kind, save those of individual capacity and experience; but the stream, whatever its volume, flows always in a determinate direction; consciousness, as we have said, is integrative." I must add that there is no passage in my book where "the image of memory and imagination . . . has been described respectively as 'discursive' and 'integrative.'" I should suppose that Mr. Davies had here made a mere slip of the pen, were it not that other remarks regarding these 'images' are equally foreign to my intention and expression.

under discussion. Such criticism is necessary, so long as we have 'schools' of psychology; and I gladly acknowledge the scientific fairness of Mr. Davies's essay. On the other hand, I think that my critic's personal views have prevented a just appreciation of my own position. For the gist of his criticism, after all, is simply this: that I have not written of Memory and Imagination as he himself would write; that I leave things out which he would include; that I emphasize features of the two consciousnesses which he regards as of minor importance. But then, so far as one may judge from articles, Mr. Davies's psychology is very different from mine. And reading the *Text-book* in the light of his own system, he has, perhaps, found difficulties and obscurities which would not arise either for a more sympathetically minded psychologist or for the beginning student.

E. B. Titchener

Cornell University

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